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I. *On the Action of Transparent Bodies upon the differently coloured Rays of Light.* By DAVID BREWSTER, LL. D. F. R. S. LOND. & EDIN. & F. A. S. ED.

(Read 5th June 1815.*)*

FROM the intimate connexion of the present subject with the improvement of the Achromatic Telescope, it must be admitted to be one of the most important in Optics; while, from the minuteness of the effects which are to be observed and compared, it is unquestionably one of the most difficult. From this cause very little progress has been made in the investigation. The irrationality of the coloured spaces, in prismatic spectra, formed by different substances, has not even been mentioned in any of our elementary treatises on Natural Philosophy, and there are some philosophers who have scrupled to receive it as a truth established in Physics.

In order to render this subject sufficiently intelligible, let us suppose that a ray of light is transmitted successively through two prisms, one of rock-crystal, and the other of flint-glass, having the same refracting angles. The rock-crystal will be found to bend the ray of light more from its primitive direction than the prism of flint-glass. The former is therefore said

to have a greater *refractive* power than the latter. If we again take other two prisms of the same substances, having such refracting angles, that a ray of light is equally refracted by both, and if we examine the spectrum which each of them affords, by admitting the sun's light into a dark chamber, it will be found that the spectrum formed by the flint-glass is much longer than the spectrum formed by the rock-crystal. The flint-glass is therefore said to have a greater *dispersive power* than the rock-crystal, or a greater power of separating the extreme rays from the mean ray of the spectrum. If we then take other two prisms with their refracting angles of such a magnitude that they produce spectra of equal lengths, it will be found by a particular mode of examination, that the coloured spaces have not the same size in the two spectra. The red and green rays will occupy more space, and the blue and violet ones less space, in the spectrum formed by the rock-crystal, than in the spectrum formed by the flint-glass. This want of proportionality, or *irrationality in the coloured spaces* of different spectra, is not of such a magnitude as to be visible upon a mere examination of the spectra themselves. In order to observe it, we must make the prism of flint-glass refract in opposition to the prism of rock-crystal; and if we look at the bars of a window through the combined prisms, we shall perceive on the side of the bar to which the vertex of the flint-glass prism is directed, a fringe of green light, and on the other side of the bar, a purple or wine-coloured fringe. The flint-glass, therefore, acts less powerfully upon the green rays than the rock-crystal, or they are less separated from the red extremity of the spectrum. These uncorrected colours, have been called the *secondary spectrum*, and form the principal obstacle to the perfection of the achromatic telescope.

In

In order to observe the secondary spectrum with distinctness, a prism of sulphuric or phosphoric acid should be made to act in opposition to a prism of flint-glass, or what is still better, to a prism of oil of cassia; the uncorrected fringes will in this case be remarkably broad and distinct, and I have seen them, when a prism of flint-glass acted in opposition to a prism of phosphoric acid, with a refracting angle of only 11°. When we look at the bars of a window through a prism of phosphoric acid, they are fringed with the prismatic colours, and so different are these colours, in their general appearance, from the colours formed by a flint-glass prism, that any person unacquainted with the subject, would immediately perceive that there was an excess in the space occupied by the orange-coloured light in the spectrum formed by the phosphoric acid. This simple experiment, may be considered as affording ocular evidence of the irrationality of the coloured spaces.

In my *Treatise on New Philosophical Instruments*, I have already published the first experiments which I made upon this subject, and I have there pointed out a method of obtaining a numerical value of the magnitude of the secondary spectrum *. Since these experiments were published, I have pursued the subject to a much greater length, and have examined almost every transparent body of importance. The results of both these sets of experiments are contained in the following pages, and are arranged in an alphabetical order, to facilitate the reference from the General Table given at the end of the Paper.

* The reader is referred to this work, p. 353—401. for farther details illustrative of this subject.

1. *Acetate of Lead, melted.*

A prism of *acetate of lead* acting in opposition to a prism of crown-glass, produces a considerable secondary spectrum, in which the green fringe is on the same side of the window bar as the vertex of the prism of acetate of lead. This last has therefore a less powerful action on the green rays than *crown-glass.*

A prism of *acetate of lead* acting in opposition to *flint-glass*, produces a less secondary spectrum, but the green fringe is still on the same side of the bar.

Owing to the imperfection of the image, I was not able to compare this substance with bodies of a higher dispersive power, as the secondary spectrum became too small.

2. *Acid, Acetic.*

The *acetic acid* acts more powerfully upon the green rays than *flint-glass.*

It acts a little more powerfully upon the green rays than *crown-glass.*

It acts less powerfully upon the green rays than *rock-crystal.*

It acts a little less powerfully upon the green rays than *muriatic acid;* but it is very difficult, in this case, to perceive the secondary spectrum.

3. *Acid, Citric.*

Citric Acid acts more powerfully upon green light than *flint-glass.*

4. *Acid, Malic.*

Malic Acid acts more powerfully upon the green rays than *flint-glass.*

It

It acts less powerfully upon the green rays than *crown-glass.*

5. *Acid, Muriatic.*

Muriatic Acid acts much more powerfully upon green light that *flint-glass.*

It acts more powerfully upon green light than *crown-glass.*

It acts less powerfully upon green light than *rock-crystal.*

It acts less powerfully upon green light than *water.*

It acts very much less powerfully upon green light than *sulphuric acid.*

6. *Acid, Nitric.*

Nitric acid acts much more powerfully upon the green rays than *flint-glass.*

It acts a little more powerfully upon the green rays than *crown-glass.*

It acts much less powerfully upon the green rays than *sulphuric acid.*

7. *Acid, Nitrous.*

Nitrous acid acts much more powerfully upon green light than *flint-glass.*

It acts less powerfully upon green light than *topaz.* (Not a good observation.)

It acts less powerfully upon green light than *rock-crystal.*

It acts less powerfully upon green light than *nitric acid.*

It has nearly the same action upon green light as *crown-glass*; an uncorrected green fringe appearing towards the vertex of the nitrous acid prism, when it is inclined to the incident ray, and towards the vertex of the crown-glass prism, when it is placed first, and inclined to the incident ray. The fringe is, however, greater in the latter case, and therefore the acid

acid may be regarded as exercising a greater action upon the green rays than the glass.

When the *nitrous acid* acts in opposition to the *muriatic acid*, no uncorrected colour is perceived.

8. *Acid, Phosphoric.*

Phosphoric Acid acts very much more powerfully upon green light than *flint-glass.* The secondary spectrum is very vivid and beautiful.

It acts much more powerfully upon green light than *crown-glass.* The secondary spectrum is very distinct.

It acts much more powerfully upon green light than the *muriatic acid.*

Its action upon green light is nearly as small as that of *sulphuric acid.* By inclining the phosphoric acid prism, the uncorrected green light appears towards its vertex; but by inclining the sulphuric acid prism, it does not appear. Hence the *phosphoric acid* acts a little more powerfully upon green light than the *sulphuric acid.*

9. *Acid, Phosphorous.*

Phosphorous Acid acts more powerfully upon green light than *flint-glass, crown-glass, rock-crystal,* or *water.*

10. *Acid, Prussic.*

Prussic Acid acts much more powerfully upon the green rays than *flint-glass.*

It acts more powerfully upon the green rays than *crown-glass.*

When it acts in opposition to *blue topaz,* no uncorrected colour is visible. The refracting angle, however of the topaz prism,

prism, was too small for ascertaining accurately the relative action of the two bodies.

It acts less powerfully upon the green rays than *water.*

When it acts in opposition to *muriatic acid*, no secondary spectrum is perceived.

11. *Acid, Sulphuric.*

Sulphuric Acid acts more powerfully upon the green rays than *flint-glass*, and affords a secondary spectrum greater than any other substance.

It acts more powerfully upon the green light than *crown-glass, muriatic acid, rock-crystal, fluor-spar, water*, and *phosphoric acid.*

12. *Acid, Sulphurous,*—water saturated with the gas.

Sulphurous Acid acts more powerfully upon green light than *flint-glass, crown-glass, rock-crystal*, and *water.*

13. *Alcohol.*

Alcohol acts less powerfully upon the green rays than *muriatic acid* and *water.*

It acts rather more powerfully upon the green rays than *flint* and *crown glass.*

When it acts in opposition to *ether*, no uncorrected colour is seen.

14. *Almonds, Oil of.*

Oil of Almonds acts more powerfully upon green light than *flint-glass.*

It acts a little less powerfully upon green light than *crown-glass.*

15. *Almonds,*

15. *Almonds, Bitter, Essential Oil of**.

This oil acts more powerfully upon green light than *oil of cassia, sulphur*, and *balsam of Tolu.*

It acts less powerfully upon green light than *oil of anise-seeds, flint-glass*, and *crown-glass.*

16. *Amber.*

Amber acts less powerfully upon green light than *crown-glass* and *rock-crystal.*

It acts a little less powerfully upon green light than *flint-glass.*

It acts less powerfully upon green light than *rock-salt.*

17. *Amber, Oil of.*

This oil acts less powerfully upon green light than *orange-coloured glass, oil of lavender, flint* and *crown glass, topaz*, and *gum-arabic.*

It acts very much more powerfully upon green light than *oil of cassia.*

It acts more powerfully upon green light than *oil of anise-seeds.*

18. *Ambergris, Oil of.*

This oil acts less powerfully upon green light than *topaz, rock-crystal, alcohol*, and *water.*

It acts more powerfully upon green light than *oil of cummin.*

19. *Anise Seeds, Oil of.*

This oil acts less powerfully upon green light than *oil of sassafras, oil of amber*, and *orange-coloured glass.*

It

* I am indebted for this oil to my friend Dr Gordon. It is a part of that which Mr Brodie used in his interesting experiments on the action of *Vegetable Poisons.*

It acts more powerfully upon green light than *oil of cassia.*

20. *Arabic, Gum.*

Gum Arabic acts more powerfully upon green light than *oil of cassia, balsam of Tolu,* and *flint-glass.*

When it opposes *crown-glass,* the uncorrected colour is scarcely perceptible; the excess of action on green light being on the side of the gum.

It acts less powerfully upon green light than *topaz* and *rock-crystal.*

21. *Beech Nut, Oil of.*

This oil acts less powerfully on green light than *crown-glass.*

It acts a very little less powerfully upon green light than *flint-glass.*

22. *Bergamot, Oil of.*

This oil acts less powerfully upon green light than *flint* and *crown glass.*

No uncorrected colour is visible when it acts in opposition to *oil of marjoram.*

23. *Beryl.*

Beryl acts more powerfully upon green light than *flint* and *crown glass.*

24. *Borax.*

Borax acts more powerfully upon green light than *flint* and *crown glass.*

25. *Glass of Borax.*

Glass of Borax acts more powerfully upon green light than *flint* and *crown-glass*.

26. *Calcareous Spar.*

Calcareous Spar acts less powerfully upon green light than *topaz* and *rock-crystal.*

It acts more powerfully upon green light than *flint-glass.*

When opposed to *crown-glass*, I have not been able to perceive distinctly any uncorrected colours.

27. *Canada Balsam.*

Canada Balsam acts less powerfully upon green light than *flint* and *crown glass.*

It acts more powerfully upon green light than *oil of cloves.*

28. *Capivi, Balsam of.*

This balsam acts less powerfully upon green light than *crown-glass.*

It acts more powerfully upon green light than *oil of marjoram.*

When it disperses in opposition to *flint-glass*, no secondary spectrum is visible.

29. *Caraway-Seeds, Oil of.*

This oil acts less powerfully upon green light than *crown* and *flint glass.*

30. *Carbonate of Lead.*

This metallic salt acts more powerfully upon the green rays than *oil of cassia.*

It

It acts very little more powerfully than the *balsam of Tolu.*

31. *Cassia, Oil of.*

Oil of Cassia acts less powerfully upon green light than *sulphur, balsam of Tolu,* and every other substance with which I have compared it. With *sulphur* the uncorrected colour is just visible.

When acting in opposition to *sulphuric acid,* it forms a secondary spectrum of a very large size; these substances being at the opposite extremities of the scale.

32. *Castor Oil.*

Castor Oil acts less powerfully upon green light than *crown-glass.*

It acts a very little less powerfully upon green light than *flint-glass.*

33. *Chamomile, Oil of.*

This oil acts less powerfully upon green light than *crown-glass.*

It acts a little less powerfully upon green light than *flint-glass.*

34. *Cloves, Oil of.*

Oil of Cloves acts less powerfully upon the green rays than *oil of lavender, Canada balsam,* and *flint-glass.*

It acts more powerfully upon the green rays than *oil of sassafras,* and *oil of cummin.*

35. *Copal, Gum.*

Gum Copal acts less powerfully upon green light than *crown-glass.*

It acts a very little less powerfully on green light than *flint-glass.*

36. *Cummin, Oil of.*

This oil acts less powerfully upon green light than *oil of lavender*, *flint-glass*, and *crown-glass.*

It acts more powerfully upon green light than *balsam of Tolu.*

37. *Diamond.*

When *diamond* acts in opposition to *oil of cassia*, the uncorrected colours are nearly of the same magnitude, and in the same situation, as when the oil of cassia is combined with flint glass. Hence *diamond* has nearly the same action upon green light as *flint-glass.*

38. *Dill Seed, Oil of.*

This oil acts less powerfully upon green light than *crown-glass.*

It acts a very little less powerfully upon green light than *flint-glass.*

39. *Egg, White of an.*

The *white of an egg* acts more powerfully upon green light than *crown-glass*, *topaz*, *muriatic acid*, *nitric acid*, and *fluor spar.*

It acts less powerfully upon green light than *water*, and much less than *sulphuric acid.*

40. *Ether.*

40. *Ether.*

Ether seems to have the same action upon green light as *alcohol.*

41. *Fennel-seeds, Sweet, Oil of.*

This oil acts much less powerfully upon green light than *flint* and *crown glass*, and less powerfully than *oil of dill seed.*

42. *Fenugreek, Oil of.*

This oil acts less powerfully upon green light than *flint* and *crown glass.*

43. *Fluor-Spar.*

Fluor-Spar acts less powerfully upon green light than *sulphuric acid* and the *white of an egg.*

44. *Glass, Crown.*

Crown-glass acts more powerfully upon green light than *flint-glass*, *oil of spermaceti*, *oil of olives*, *oil of ambergris*, *copal*, or *oil of juniper.*

It acts less powerfully upon green light than *gum Arabic*, *alcohol*, *topaz*, *fluor-spar*, the *acids*, *rock-crystal*, and *water.*

45. *Glass, Flint.*

Flint-glass acts less powerfully upon green light than *oil of spermaceti*, *oil of olives*, *oil of ambergris*, *essential oil of juniper*, *oil of rape-seed*; *crown-glass*, &c.

It acts more powerfully upon green light than *Canada balsam*, and almost *all the essential oils*, excepting those expressly mentioned.

When

When flint-glass acts in opposition to *balsam of Capivi*, *nut oil*, *oil of rhodium*, and *oil of rosemary*, no secondary spectrum is visible.

46. *Glass, Opal-coloured.*

This glass acts more powerfully upon green light than *oil of cassia*, and *balsam of Tolu*.

It acts less powerfully upon green light than *flint-glass*.

47. *Glass, Orange-coloured.*

This glass acts less powerfully upon green light than *crown-glass*, and very little less than *flint-glass*.

It acts more powerfully upon green light than *oil of amber*, and *oil of anise-seeds*.

48. *Glass, Red-coloured.*

This glass acts less powerfully upon green light than *flint* and *crown glass*.

49. *Hyssop, Oil of.*

This oil acts less powerfully upon green light than *crown-glass*, and a little less powerfully than *flint-glass*, and *oil of peppermint*.

50. *Ice.*

When *ice* is opposed to *flint-glass*, it forms a secondary spectrum, having the same position, and nearly the same magnitude, as when *water* is combined with *flint-glass*.

51. *Juniper, Gum.*

This gum acts less powerfully upon green light than *crown-glass*.

52. *Juniper,*

52. *Juniper, Oil of.*

This oil acts less powerfully upon green light than *crown-glass.*

It acts a very little more powerfully upon green light than *flint-glass.*

53. *Lavender, Oil of.*

This oil acts less powerfully upon green light than *flint* and *crown glass.*

It acts more powerfully upon green light than *oil of cassia, balsam of Tolu,* and *oil of amber.*

54. *Lemon, Oil of.*

This oil acts less powerfully upon green light than *crown-glass,* and a very little less powerfully than *flint-glass.*

55. *Leucite.*

This mineral acts more powerfully upon green light than *flint* and *crown glass.*

It acts less powerfully upon green light than *topaz.*

56. *Marjoram, Oil of.*

This oil acts less powerfully upon green light than *flint* and *crown glass,* and *nut oil.*

57. *Muriate of Antimony.*

This fluid, (the dispersive power of that which I used was 0.059,) acts far less powerfully upon green light than *flint* and *crown glass.*

It acts more powerfully upon green light than *oil of sassafras.*

It

It exercises almost the same (perhaps a little greater) action upon green light, as *oil of cloves.*

58. *Nitrate of Potash.*

The refractive force which produces the greatest refraction in this salt, acts much less powerfully upon green light than *crown-glass*, and a little less powerfully than *flint-glass.*

59. *Nut Oil.*

Nut oil acts less powerfully upon green light than *crown-glass.*

When it acts in opposition to *flint-glass,* no secondary spectrum is visible.

It acts more powerfully upon green light than *oil of marjoram.*

60. *Nutmegs, Oil of.*

This oil acts less powerfully upon green light than *crown-glass*, and a very little less than *flint-glass.*

61. *Olives, Oil of.*

Oil of olives acts less powerfully upon the green rays than *crown-glass*, and a very little less than *flint-glass.*

62. *Pennyroyal, Oil of.*

This oil acts much less powerfully upon green light than *crown-glass*, and considerably less than *flint-glass.*

63. *Peppermint, Oil of.*

Oil of peppermint acts less powerfully upon the green rays than *crown-glass*, and a little less powerfully than *flint-glass.*

64. *Poppy,*

64. *Poppy, Oil of.*

This oil acts less powerfully upon green light than either *flint* or *crown glass.*

65. *Rape-seed, Oil of.*

This oil acts less powerfully upon the green rays than *crown-glass,* and a very little more powerfully than *flint-glass.*

66. *Rhodium, Oil of.*

This oil acts less powerfully upon green light than *crown-glass,* and more powerfully than *oil of cloves.*

When a prism of oil of rhodium with a great refracting angle, was opposed to a prism of flint-glass with a small angle, the green fringe appeared towards the vertex of the flint-glass prism.

When a prism of oil of rhodium with a small refracting angle, was made to correct a prism of flint-glass with a large angle, the uncorrected green appeared towards the vertex of the oil of rhodium. Hence we may infer that these two bodies exercise the same action upon the green rays.

67. *Rock-Salt.*

This substance exercises a much greater action upon green light than *rosin.*

It exercises a less action upon the green rays than *crown-glass.* Its action exceeds a little that of *flint-glass,* and is a little less than that of *calcareous spar.*

68. *Rock-Crystal.*

This mineral acts more powerfully upon green light than *flint-glass, crown-glass,* and *muriatic acid.*

It acts less powerfully upon green light than *sulphuric acid, phosphoric acid* and *water.*

69. *Rosemary, Oil of.*

This oil acts less powerfully upon green light than *crown-glass,* and a very little more powerfully than *flint-glass.*

70. *Rosin.*

This substance acts less powerfully upon green light than *crown-glass* and *flint-glass.* The secondary spectrum with flint-glass, is fully as great as that produced by flint-glass opposed to crown-glass.

Rosin acts a little less powerfully upon the green rays than *oil of cloves.*

71. *Rue, Oil of.*

This oil acts more powerfully upon the green rays than *crown-glass,* and a very little more powerfully than *flint-glass.*

72. *Sage, Oil of.*

This oil acts less powerfully upon green light than either *crown* or *flint glass.*

73. *Sassafras, Oil of.*

This oil acts more powerfully upon green light than *oil of cassia, balsam of Tolu,* and *oil of anise-seeds,* but less powerfully than *crown* and *flint glass,* and *oil of cloves.*

74. *Savine, Oil of.*

This oil acts less powerfully upon the green rays than *crown-glass,* and perhaps a little less powerfully than *flint-glass;* but more powerfully than *oil of spearmint.*

75. *Selenite.*

75. *Selenite.*

This mineral acts more powerfully upon green light than *crown* and *flint glass*, but less powerfully than *topaz*.

76. *Spearmint, Oil of.*

This oil acts less powerfully upon green light than *flint* and *crown glass*, and *oil of savine*.

77. *Spermaceti, Oil of.*

This oil acts more powerfully upon the green rays than *flint-glass* and *oil of marjoram*, and less powerfully than *crown-glass*.

78. *Sulphur.*

Sulphur produces a secondary spectrum when opposed to *flint-glass* and *crown-glass*, nearly as great as when *oil of cassia* is opposed to them. When it refracts in opposition to *oil of cassia*, no uncorrected colour is visible, but the oil appears to have a less action upon the green rays.

79. *Sulphuret of Carbon.*

This fluid acts much less powerfully upon the green rays than *flint* and *crown glass*, and all the essential oils except *oil of cassia*.

Its action does not greatly exceed that of the *oil of cassia*.

80. *Super-sulphuretted Hydrogen.*

This fluid has a more powerful action upon the green rays than *flint* and *crown glass*. When it refracts in opposition to *water*, no uncorrected colour is visible.

81. *Tartrate of Potash and Soda.*

This salt acts more powerfully upon green light than *flint-glass*, and does not appear to act very differently from *crown-glass.*

82. *Thyme, Oil of.*

This oil acts less powerfully upon green light than *flint* and *crown glass*, and *oil of juniper*, but more powerfully than *oil of cloves.*

83. *Tolu, Balsam of.*

This balsam acts less powerfully upon the green rays than *oil of anise-seeds*, *oil of sassafras*, and *oil of lavender*, and *flint-glass*, but more powerfully than *oil of cassia* and *sulphuret of carbon.*

84. *Topaz, Blue.*

This mineral acts more powerfully upon green light than *crown* and *flint glass*, *selenite*, and *leucite*, but less powerfully than *water* and *rock-crystal.*

85. *Tourmaline.*

This mineral acts more powerfully upon green light than *crown* and *flint glass.*

86. *Turpentine, Oil of.*

This oil acts more powerfully upon the green rays than *oil of sassafras*, *oil of cloves*, and *Canada balsam*, but less powerfully than *crown* and *flint glass.*

87. *Water.*

87. *Water.*

Water acts more powerfully upon the green rays than *flint* and *crown glass*, *rock-crystal*, *the white of an egg*, and the greater number of the acids. It acts less powerfully, however, than the *phosphorous*, *sulphurous*, *phosphoric* and *sulphuric acids*.

88. *Wormwood, Oil of.*

This *oil* exceeds *flint* and *crown glass* in its action upon green light, but is inferior to *oil of sassafras* and *oil of cloves*.

89. *Zircon.*

This mineral acts less powerfully upon green light than *crown-glass*, but no secondary spectrum was visible when it refracted in opposition to *flint-glass*.

In order to present under a general view the results of the preceding experiments, I have arranged the various substances in a Table, inversely according to their action upon green light; that is, the bodies at the top of the Table form spectra in which the red and green spaces are most contracted, and the blue and violet ones most expanded. The relative position of several of the substances, particularly the essential oils, is quite empirical; but it can readily be found by a reference to the preceding experiments, whether or not the relative action of any two bodies has been actually determined. To have attempted to fix the exact place of every substance, would have required a great degree of labour, and more time than I could easily command; and it is almost certain, that the Table would still have been incomplete, as there are many of the essential oils, and some minerals, which do not exhibit any difference in their action upon green light. Several of the oils, too, which I used, had been exhausted by repeated

peated trials, and as it would have been impossible to replace them from the same source, the prosecution of the experiments with fresh oils, sold under the same name, might have introduced a new degree of uncertainty among the results.

For all the purposes of the Practical Optician, the relative position of the substances, where it has been determined by experiment, is sufficiently accurate. Numerous combinations for correcting the secondary spectrum, may be formed, either by taking two media that have nearly the same action upon green light, while they differ in their powers of refraction and dispersion, or by adopting the ingenious method discovered by Dr Blair, and fully described in the *Transactions* of this Society. In the production of perfect achromatic instruments, the optician cannot expect much more aid from the principles of optics. The great, and almost the only desideratum, is to obtain two kinds of glass, which shall have the same action upon green light, while they differ sufficiently in refractive and dispersive power; and it is chiefly from the labours of the Chemist, guided by the preceding inquiries, that such a discovery can be expected.

There is still, however, another source of error, of which neither the Theoretical nor the Practical Optician has hitherto been aware. It arises from a crystallisation in the glass, which is always accompanied with double refraction, and with a variation of density. This crystallisation, which most frequently affects the flint-glass, and which can easily be detected by its action upon polarised light, should be carefully removed from every piece of glass used in the construction of optical instruments, by annealing it in an oven of a high temperature, where the heat is regularly and very slowly reduced. The experiments upon which this opinion is founded, will form the subject of another paper.

TABLE

TABLE of Transparent Bodies, arranged inversely according to their Action upon Green Light.

1 OIL OF CASSIA.
Sulphur.
Sulphuret of carbon.
Balsam of Tolu.
5 Carbonate of lead.
Essential oil of bitter almonds.
Oil of anise-seeds.
Oil of cummin.
Oil of sassafras.
10 Oil of amber.
Acetate of lead melted.
Opal-coloured glass.
Orange coloured glass.
Red-coloured glass.
15 Oil of sweet fennel seeds.
Oil of cloves.
Muriate of antimony.
Oil of lavender.
Canada balsam.
20 Oil of turpentine.
Oil of sage.
Oil of pennyroyal.
Oil of poppy.
Oil of hyssop.
25 Oil of spearmint.
Amber.
Oil of lemon.
Oil of caraway-seeds.
Oil of nutmegs.
30 Oil of thyme.
Oil of peppermint.
Oil of bergamot.
Oil of marjoram.
Oil of wormwood.
35 Oil of dill-seeds.
Oil of chamomile.
Castor-oil.
Gum copal.
Rosin.
40 Diamond.
Nitrate of potash.
Oil of beech-nut.
Oil of rue.
Oil of savine.
45 Nut oil.
Balsam of capivi.
Oil of fenugreek.
Oil of rosemary.
Oil of rhodium.
50 FLINT GLASS.
Zircon.
Oil of olives.
Oil of rape-seed.
Oil of spermaceti.
55 Oil of juniper.
Oil of ambergris.
Calcareous spar.
Rock-salt.
Gum juniper.
60 Tartrate of potash and soda.
Oil of almonds.
CROWN GLASS.
Gum-Arabic.
Alcohol.
65 Ether.
Borax, glass of.
Borax.
Tourmaline.
Leucite.
70 Selenite.
Beryl.
Topaz, blue.
Fluor-spar.
Citric acid.
75 Malic acid.
Acetic acid.
Nitrous acid.
Muriatic acid.
Prussic acid.
80 Nitric acid.
Rock crystal.
White of an egg.
Ice.
WATER.
85 Super-sulphuretted hydrogen.
Phosphorous acid.
Sulphurous acid.
Phosphoric acid.
89 SULPHURIC ACID.

II.

II. *Description of a New Darkening Glass for Solar Observations, which has also the property of polarising the whole of the transmitted Light.* *By* DAVID BREWSTER, LL. D. F. R. S. LOND. & EDIN. & F. A. S. ED.

[*Read 1st May* 1815.]

IT will be readily admitted by every person who has been accustomed to solar observations, that an apparatus for diminishing the intensity of the sun's light, without distorting or colouring the resulting image, is still a desideratum in Practical Astronomy.

Dr HERSCHEL is the only person who has given any degree of attention to this subject. When he applied his powerful telescopes to examine the surface of the sun, he found that the ordinary method of attenuating the light by smoked or coloured glasses, was of no avail; and it was in the prosecution of his experiments for determining the relative advantages of differently coloured glasses, or of combinations of differently coloured glasses, that he was conducted to those splendid discoveries respecting the invisible rays, which have formed an epoch both in Chemistry and Optics.

 The

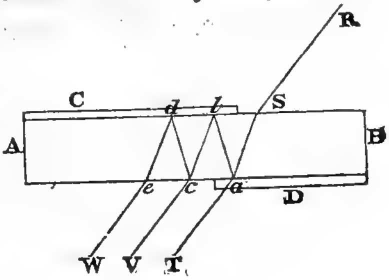
The combination of coloured and smoked glasses which he found most effectual for diminishing the heat of the sun's rays, and at the same time preserving distinct vision, was extremely complicated; and after every attempt to improve it, he seems to have preferred another apparatus in which the light was transmitted through a mass of diluted ink contained in a trough bounded by plates of parallel glass. By this means he obtained an image of the sun as white as snow, and so very distinct, that, with a mirror nine inches in diameter, and the eye-piece open, he could observe the sun in the meridian for any length of time, without injuring either his eye, or the glasses of his eye-piece.

Notwithstanding these advantages, this apparatus can never be brought into general use, and can only be employed in instruments that are stationary. The necessity of frequently renewing the ink, and the difficulty of retaining it in the trough, are evils which are not easily remedied.

The darkening glass which we propose to substitute in place of these contrivances, depends upon the diminution of light by two or more reflexions within a thick plate of parallel glass. The pencil, attenuated by reflexion, reaches the eye in the state of white light, while the direct rays transmitted through the plate are stopped by two pieces of metal properly disposed upon the opposite surfaces of the parallel glass.

The nature of this contrivance will be understood from the following figure, where AB is a section of a piece of parallel glass, and C, D sections of two opaque plates placed on the upper and under surfaces of this glass. A ray of light RS, incident obliquely at S, will be transmitted in the direction *a* T, but is prevented from reaching the eye by the opaque plate D. A portion of this ray is reflected at *a*, and a second time at *b*, and emerges in the direction *c* V in a very attenuated state. The intensity

sity of the pencil cV may be varied either by varying the thickness of the plate, or by changing the angle of incidence; and if it cannot be rendered sufficiently feeble by any of these means, it may be exposed to other two reflections at c and d, when it will emerge in the direction eW, having its intensity very greatly reduced.



In order that the light may be freely reflected at the points a, b, c and d, the opaque plates C, D must be kept at a little distance from the surface of the glass, and all extraneous reflexion must be removed, by covering their interior surfaces with a black pigment. This may be done most conveniently by making the plates C, D rest upon the glass only by their margins. The aperture at S, where the light is introduced, should be of an elliptical form, so as to admit a cylindrical pencil at an oblique incidence.

Hitherto we have supposed, the reflecting surfaces to be in contact with air, so that the reflecting force is allowed to exercise its maximum action upon the incident rays. But it is very easy to diminish the reflective power, in any ratio that we choose, by introducing between the opaque plates and the glass a cement either of a greater or a less refractive power than the glass. By this means we obtain a degree of reflexion corresponding to a refractive power equal to the quotient of the greater refractive power divided by the lesser; and the reflexion will be made either from the surface of the cement, or from the glass, according as the one or the other exercises the most powerful action upon light.

As it may sometimes be difficult to procure a thick plate of parallel glass, we may substitute in its place two common

plates,

plates, cemented together by a thin layer of indurated Canada balsam, or any other transparent substance, having the same refractive power. The rays of light will then pass from the one plate into the other, without suffering either reflexion or refraction. This compound plate may even be formed of glasses of different colours, if we wish to produce a great degree of attenuation.

The simple apparatus which has now been described, possesses a still more valuable property than that of attenuating the incident light. The pencil c V, which has undergone two reflexions, emerges completely polarised, unless when the angle of incidence is very small; and the polarisation continues complete, although this angle suffers a very considerable variation. The other pencil e W is also polarised, and preserves this character, even when the angle of incidence has a much wider range.

If a plate of fine flint-glass is used in the construction of the eye-piece, we may employ it to great advantage in experiments on polarisation, even when the light has a moderate degree of intensity. But if the light of the sun is under examination, the eye-piece will possess the peculiar advantage, of at the same time attenuating and polarising the incident pencil.

The polarisation of the emergent pencils c V, e W, enables us to explain a very perplexing anomaly in the law of the polarisation of light by oblique refraction *. From numerous experiments made with piles of glass plates, I found that the tangents of the angles at which they polarised the transmitted light, were inversely as the number of plates of which the pile was composed. The coincidence of this law, with the experimental results, was extremely accurate when the number of plates was between *eight* and *forty-seven*, corresponding to a series

* See the *Philosophical Transactions*, for 1814, p. 223.

series of angles between 41° 41′ and 79° 11′. When the light was transmitted through one plate, it ought to have been polarised, according to the preceding law, at an angle of 88° 38′; but upon examining the image with a prism of calcareous spar, it did not vanish in any position of the spar, even when the incidence was greater than 88° 38′. A similar aberration from the law takes place when the pencil is transmitted through *one*, *two*, *three*, or *four* plates.

In order to discover the cause of this anomaly, I made a great variety of experiments, without obtaining any satisfactory result; and it was only by examining the phenomena of the polarising eye-piece that my attention was again called to the subject. We have already seen, that the pencils *c* V and and *e* W, are polarised in the plane of reflexion S*abcde*. If we now remove the lower plate D, so as to permit the direct pencil *a* T to reach the eye, the pencils *c* V, and *e* W will be mingled with the pencil *a* T. When RS is incident at an angle of 88° 38′, the pencil *a* T ought to be completely polarised, and ought to vanish in every quadrant, when examined with a polarising crystal. It is prevented, however, from vanishing, by the admixture of the pencils *c* V and *e* W, which are polarised in an opposite manner, and which will therefore remain visible when the whole of the direct pencil has vanished. As the angle of incidence diminishes, the intensity of the pencils *c* V, *e* W suffers a very rapid diminution, while that of the direct pencil *a* T receives a corresponding augmentation. On this account, the oppositely polarised rays *c* V, *e* W form but a very small proportion of the compound pencil, and they are scarcely perceptible when the number of plates exceeds eight. The union of the reflected light with the obliquely refracted pencil, constitutes, therefore, the true cause of the anomaly which we have been considering.

III.

III. *Observations on the Fire-Damp of Coal Mines; with a Plan for Lighting Mines, so as to guard against its Explosion.* *By* JOHN MURRAY, M. D. F. R. S. E. *Fellow of the Royal College of Physicians of Edinburgh.*

[*Communicated November* 14. *and read November* 20. 1815 *.]

EXPLOSIONS in mines, from the kindling of the inflammable gas, called *Fire-Damp* by the miners, have always occasionally occurred. Of late they have become more frequent in some of the coal-mines in this country, particularly those in the districts of the Tyne, and the Wear, in the North of England, and have been attended with such fatal consequences, as to have forcibly called public attention to the subject. In an explosion in one mine, about two years ago, ninety-two persons were killed; in another, which occurred soon after, thirty-two lost their lives; in one which happened within these few months, fifty-seven persons were destroyed; and recently, it has been affirmed, that several hundred lives are lost annually

* It may be proper, from circumstances, to mention, that this paper is printed in the text exactly as it was read. I have added at the end a few notes, (read before the Society at a subsequent meeting), explanatory of the plan, or connected with the subject.

annually from this cause. From the state of the mines, particularly in the accumulation of wastes, the collection of water, and the increasing depth of the workings, there is reason to fear, too, that such accidents will become more frequent. Humanity loudly calls, therefore, on every effort being made to obviate the calamity; and even as a national concern, the immense loss of property in the mines, and the probability which has been suggested, that the working of them must ere long be abandoned, give to the subject the highest claims to consideration.

I have to submit to the Society the account of a method which has occurred to me of lighting mines, not liable, like the common method, to the risk of kindling the fire gas, and which I trust may go far to obviate these unhappy occurrences. (Note A.)

The inflammable gas which is disengaged in coal-mines, it is well known, is carburetted hydrogen. In some situations it is much more abundant than in others. It has been supposed to be produced from the decomposition of water by coal, and, in particular, from the waste coal in the old workings, exposed to the action of humidity. It is possible that much of it may be from this source, and the fact, that it is most abundant in deep mines, where such wastes accumulate, is favourable to the opinion. This is not always, however, its origin. Much of it is disengaged from the solid coal as it is worked, and the surface of the wall of coal often continues to yield it from pores or fissures for weeks or months. It often, too, rushes suddenly, with great velocity, and in large quantities, from rents in the incumbent strata, or from vacuities within the mass of coal, in which it is pent up, apparently in a state of compression. The greatness of the mass of coal confining the gas more effectually, or favouring the compression with which it is retained,

ed, may be the cause why it is more abundant in deep mines. Still, from whatever source it may be extricated, if the fact be correctly stated, that it has always an intermixture of carbonic acid, it is probable that it is in all cases derived from the decomposition of water by coal. (Note B.)

It seems to be altogether impracticable to prevent the production of this gas. To decompose or neutralise it in the mine by any chemical agency, as has been suggested, seems to be equally so. Only two resources apparently remain,—first to employ means of discharging it, so as to prevent any accumulation of it in large quantities; and, secondly, to guard against its inflammation in working the mine, when from circumstances occasionally unavoidable it does accumulate, until it can be discharged.

The circumstance of explosions from the firing of this gas not occurring in many mines, situated even in districts where, from the nature of the coal, the depth of the workings, or other causes, such occurrences are frequent, seems to prove that there is no necessary accumulation of fire damp in the mine,—that the accumulation does not so much take place from unavoidable deficiency of ventilation, as from accidental obstructions to it in particular situations, or occasional eruptions of the gas from cavities in the coal or its accompanying strata, against which scarcely any system can effectually guard,—that, therefore, the ventilation may be rendered effective, and, by prudent and careful management, may be conducted so as to carry off the quantities evolved. In the Felling Colliery, near Newcastle, in which the explosion that destroyed nearly a hundred persons, about three years ago, took place, the mine, it is stated, was considered as a model of perfection in the purity of its air, from the system of ventilation; and in an account of a second explosion in this mine, in which

 twenty-

twenty-three were killed, it is mentioned, "that so powerful was the stream of fresh air in all the working parts of the mine, that the candles could with difficulty be kept from going out," and "that the persons employed in it declared, that they never wrought in a pit so wholesome and pleasant." In another mine, in which, in the same year, an explosion took place, in which thirty-two men were killed, the general arrangements were so perfect, that it was considered by every one acquainted with the state of it, to be altogether free from danger. These facts seem to shew, that there is no want in the power of ventilation; and indeed it has been stated on high practical authority, that in this respect no great improvement can be expected *. If these statements are correct, what is principally to be looked for, independent of employing the best method of ventilation, and of a more strict attention to the state of the mine, in preventing any partial obstruction to its operation, is some mode of security against the inflammation of the gas, either as it is discharged from the fissures of the coal in working, or when it does accumulate partially, from causes frequent, though occasional, in their occurrence.

So far as can be learnt from the circumstances of those explosions which have occurred in the Newcastle and Sunderland mines, the principal causes giving rise to the accumulation of the inflammable gas, have been some neglect with regard to the means of ventilation,—such as failing to keep up the fire sufficiently at the mouth of the air shaft, or obstructions in the passages or in the old wastes. The latter appear to be the most common cause; parts of the roof fall in, in the old excavations, or, by a yielding at the bottom of the pillars and

* Report by Mr Buddle to the Society for preventing Accidents in Coal-mines.

and walls, the sides of the passages gradually approach, forming what the miners call a *creep*; sometimes, too, the stoppings and trap-doors which direct the current of air through the passages, are neglected, the ventilation is either partially stopt, or is impeded, the fire-damp gradually accumulates, and mingling with the air, forms a mixture which is capable of exploding. There is reason to believe, too, that in some cases, the accident has arisen from the sudden discharge of the gas from fissures in the strata, or from the opening of a cavity in the mass of coal in which it had been confined; it often takes fire at the candles of the miners, when discharged in this way; is sometimes discharged in large quantity with the greatest violence; and if intermingled with a rapid current of atmospheric air, the inflammation may increase in rapidity to an explosion.

It is obvious, that attention to these circumstances is of the first importance; and, so far as improvements in the system of the mines in that respect are practicable, their propriety cannot be questioned. As it appears, however, that there are some causes which can scarcely be effectually obviated, and as the utmost attention which can reasonably be expected, seems, under the circumstances of the mines, and the constant generation of the enormous quantities of gas which they yield, to be insufficient for perfect security, the importance becomes evident, of some mode of lighting being devised, which should guard against the firing from the large discharge, or occasional accumulation of the inflammable air, while, at the same time, the danger should be indicated, so that the necessary means to remove it might be employed.

No difficult or complicated method can be expected to succeed. Any method to be successful, must be simple, easy of execution and of use, and not too expensive. That which I

have now to explain, will be found, I trust, possessed of these advantages.

The facts on which it is in a great measure founded, are, that the inflammable gas accumulates in the roof of the mine, —that it is fired, in the usual mode of lighting, before the mixture of it with the atmospheric air fills the mine, or that part of it in which the accumulation is taking place,—and that it cannot fill it while the mine is worked, as the respiration of the workmen would be previously affected. The miner works with his candle or lamp at a certain elevation, occasionally moving with it; and thus when the fire-damp has accumulated so far as to fill a considerable part of the roof, the accidental approach of the lamp, or some concussion throwing the gas downwards, so as to bring it into contact with the flame, sets it on fire. In one of the explosions, for example, within these two years, that of the Hall Pit near Sunderland, in which thirty-two men were killed, the explosion was supposed to have been occasioned by the fall of a stone from the roof, which carried the inflammable air with it, so as to bring it into contact with the pitmens' candles; and this circumstance of a flake or mass falling from the roof, and throwing the inflammable air before it to the candles, has been often assigned as a cause of these explosions. It is a proof of what is indeed sufficiently established, the accumulation of the inflammable gas in the roof of the mine.

The method, therefore, which I would propose is, to bring the supply of air to sustain the combustion of the lamp from the floor of the mine. This may be easily done by burning the lamp within a glass-case, having a small aperture at the top to admit of the escape of the heated air and smoke, and having attached to the under part of it a tube reaching to the floor of the mine to convey the air. Figure 1st, Plate I. represents this in a fixed lamp.

One

One principal difficulty in contriving any safe mode of lighting coal-mines, must be that of having moveable lights; for these the workmen will often find it necessary, and in general will be desirous, to employ. This may be easily attained by connecting with the bottom of the case or lantern, a flexible tube, air-tight or nearly so, which may be done by a tube of prepared leather varnished; this tube being of such a length as to reach nigh to the floor. The lamp can thus be held in the hand, or attached to any occasional support. Fig. 2. represents a lamp of this kind.

No danger, or scarcely any, I conceive, can arise in the use of this apparatus. If the size of the upper aperture be duly adjusted, no air can enter by it to the lamp, for the current of heated air will prevent this. And this air can never be heated so high as to kindle any mixture of carburetted hydrogen.

No inflammable air can enter from the bottom, so as to be capable of kindling, for the reason already assigned, that from its levity it rises to the roof of the mine, and the mixture of it with atmospheric air, which is explosive, is always accumulated there. Nor can this increase so as to extend to the floor of the mine, and the miner remain present, as, previous to this, the effect of this mixed air, received by respiration, would be felt, and give warning of the danger. (Note C.)

The flexible tubes of the moveable lamps may be easily prepared, and preserved air-tight; and there being so ready a supply of air from below, if there were any minute fissure in the sides of the tube, no air would enter by it, or the quantity would be so small, and so much diluted by intermixture, that there could be no risk. In the fixed lamps, having an iron or copper tube conveying the air, there could be no risk of this kind, and if it were necessary, similar metallic tubes, with moveable circular joints, could be adapted to the moveable lamps.

There

There is another circumstance which will give security to these lamps, should it ever happen, from any unforeseen cause, that a mixture of inflammable air were introduced,—the rarefaction of the air within the lamp, and especially near the flame. It is well known that mixtures of inflammable air with atmospheric air, or even with oxygen, cannot be inflamed, if the elastic fluid be in a certain degree of rarity. The experiments of Grotthus with regard to this are important. They prove that the combustibility of the inflammable gases is so much dependent on their density, that if a mixture of any of them with oxygen gas be rarefied to a certain extent, either by the air-pump, or by elevation of temperature, it could not be kindled by the electric spark which kindled the same mixture easily in its denser state. Hence, as he justly remarked, bodies may be inflammable under pressure, the inflammability of which is weak, or not apparent in a rarefied atmosphere; and in mixtures of different inflammable gases with atmospheric air, there will be a certain degree of density within which only the mixture can be inflamed. The inflammability of any mixture of carburetted hydrogen with atmospheric air, is limited to certain proportions, and in all of them is inconsiderable. Dr Thomson states what is a proof of this, that he had never been able to cause any mixture of it with atmospheric air to explode, it merely burnt rapidly; its exploding in the mine, must therefore probably be owing to the large mass of it inflamed, and to the state of condensation in which it exists. Another circumstance which shews, that even in the mine, its power of kindling so as to explode, is not more than what just renders it possible, is, that it is not kindled by the ignited sparks from the collision of steel and flint, a machine producing these being used to give light in working or exploring the mine, when much danger is dreaded, and having very seldom caused explosion.

sion. Thus coming barely within the verge of the power of exploding, and owing it to these circumstances of quantity and condensation, there is every probability, that if kindled in small quantities it would not explode, and that, presented to an ignited body much diluted and in a rarefied state, it would not even inflame. Hence the chance of its inflaming in a lamp such as that described, is inconsiderable, were it even admitted to the flame; and the certainty of its not exploding might almost be depended on. The security from this might even be carried so far, by adapting properly the size of the upper aperture, so as to produce the greatest degree of rarefaction in the air, that if a mixture of the fire-damp with atmospheric air were introduced, it might, instead of inflaming, become incapable of supporting the combustion, or at least might so far weaken the flame, as to give indication of the danger.

Lastly, If even, from some singular cause, an explosion did happen within the case of the lamp, it appears to me very doubtful if it would be propagated farther. It must be extremely feeble. The flame or ignition could not be communicated by the upper aperture to the air without, partly from its smallness, and partly from the upper part of the lamp being previously occupied by air not capable of kindling. Nor could it be easily communicated downwards through the tube, especially in a flexible tube, the sides of which would first yield, and then collapse; and though conveyed downwards, there would be little or no probability of an air occupying the floor of the mine capable of being inflamed. The risk of such a communication, if it were thought there were any, might probably also be diminished by conveying the air rather by several small tubes than by one larger. Other contrivances sufficiently simple, if they were supposed necessary, might be employed. A cup containing water, for example, or a saline solution,

lution, might, in situations peculiarly hazardous, be suspended within the case, over the orifice of the air tube, which, if any explosion were to happen, would, by the agitation, throw out the fluid, and extinguish the flame.

If, notwithstanding all these means of safety, there should in any particular case be any dread of danger from the admission of inflammable air with the common air, this might be completely obviated by an additional arrangement. The air to supply the lamps might be brought by a cast-iron pipe from any part of the mine where the danger did not exist, or, what would give entire security, from the bottom of the shaft, where the air must be pure. A pipe or pipes of this kind running through the principal passages, small upright tubes might arise from it at convenient distances to the fixed lamps. A similar mode might be extended even to the moveable lamps; the flexible tube attached to the lamp might be of such a length as to reach to a part of the mine where the air was known to be pure; or such flexible tubes might be adapted to branches fitted with stop-cocks, and communicating with the main trunk. Thus a system of perfect security, I conceive, would be attained. Independent of the other circumstances diminishing any hazard, there are here only two modes of communication with the external air, from neither of which can any danger arise,—by the upper aperture, and the lower tube. The former can admit no air to the lamp within, and the latter must convey merely pure air. Both, therefore, must be perfectly safe.

I have stated these diversities of method, merely to shew how far the plan may be carried, where particular situations require it; and absolute security attained: but it is very probable, that in general they will be unnecessary, and that the simple mode first explained, of a tube connected with the lamp, supplying air from the floor, will be sufficient.

This

This method, in its simplest form, affords another security, that of avoiding the igniting of any stream or blast of inflammable air, as it issues from the coal. This it often does suddenly, and with great violence. A current of this air being kindled in the common mode of lighting the mines, by the approach of a candle or lamp, is a frequent cause of explosions, the stream of flame extending to the mixture of inflammable air and common air in the roof of the mine, and causing it to explode. The inclosing of the lamp in the case, of course, prevents any accident of this nature.

By these arrangements, adapted more or less to circumstances and situations, a system of lighting mines may be established, I trust, perfectly safe, with any common care. And the extreme simplicity of the plan, facility of execution, and economy, are recommendations in its favour.

It is scarcely necessary to enter on the details of the modes of construction of the apparatus, as these are both obvious, and admit of considerable diversity. I have given what appears to be the best figure of the glass case for a fixed lamp, sufficiently wide to prevent it from being broken by the heat, and not too much so, so as to lessen the current and rarefaction of the air. The aperture, too, is adjusted to the same purpose. If a lamp with oil be employed, it can either be suspended from the top, or fixed on a socket from beneath; if a candle be used, it must have a socket, which it may be requisite should be a sliding one, to adjust it to the due height, as it burns down. The glass cases may be protected from external injuries by a wire netting. Fixed lamps will in general be less exposed to risk than moveable lights, and by employing them in sufficient number, few of the latter may be required. In all the passages of the mine the former may be employed; and in all cases where fixed lamps with a metallic tube reaching nigh to the floor can be introduced, the method is more simple, and

 is

is attended with less trouble, and requires less attention than any other that can be used.

Some precautions may be necessary in kindling the candles or lamps. In the moveable apparatus this may be done at the bottom of the shaft, or any other part of the mine where the air must be pure. The fixed lamps may be kindled in a similar manner, being supplied with a flexible tube at the bottom, to be removed when they are transferred to the fixed tube, and, if necessary, with the additional precaution of stop-cocks to each. Various arrangements with regard to these will readily occur. (Note D.)

I have said that the accumulation of the carburetted hydrogen in the air of the mine, may be discovered by its effect on respiration. Its deleterious agency is well known. At the same time, as in the greater number of situations, its addition to the air must be gradual, it will not exert its full deleterious power, but produce only such effects as will give warning of its presence.

Even before it acts this far, it will be apparent by its smell. Hydrogen in a humid state has a sensible odour. The fire-damp in mines is known by its smell; the miner, in judging of its presence, always advancing with considerable confidence when no smell is to be perceived; and this criterion must become still more evident, when the person exposed to it is guarded from its early explosion, and it is thus allowed to accumulate to a greater extent. It is also sometimes apparent to the eye, by the vapour which is diffused through it, forming a kind of mist, floating under the roof of the mine, and fluctuating with every movement of the air.

There is one other circumstance which has been employed as a criterion, though, in the usual mode of applying it, it seems to be a very hazardous one,—what is called the *candle*

top.

top. This is a yellowish-coloured diffusion of the light round the flame of the candle, rising higher, and assuming a greenish-blue colour when the quantity is considerable, and when it is still larger, giving rise to a rapid succession of luminous points or flashes. The miner, when judging from this of the presence of fire-damp, advances cautiously in the mine, observing the appearance on his candle, by raising it slowly from a certain height from the floor,—a circumstance I may remark which shews very well the tendency to the accumulation of the inflammable gas in the roof, and the comparative purity of the air below. He thus advances, as far as he can with safety, and it is singular to what length the miners sometimes proceed with this trial. This peculiar appearance seems to arise partly from the extinguishing effect of the reduced air on the flame of the candle; for a similar effect is produced in immersing a lighted candle slowly in any gas unfit to sustain combustion; and as it extends so high, it must also partly arise from the imperfect ascension of the inflammable gas. If any inflammable air were to enter the lamp I have described, this appearance, or something similar to it, would take place, even sooner, from the rarefaction of the air, and probably the flame would be entirely extinguished before any explosion occurred. It would therefore give an equally sure indication with much more safety; though it is also probable, that no air, so far impregnated with carburetted hydrogen, could ever enter from the floor of the mine while the atmosphere above was such that it could be breathed.

When the danger is suspected to exist, the state of the air in any part of the mine may be accurately examined with great facility, and entire safety. With a moveable lamp and tube, such as has been described, or, if it were thought to give more safety in extreme cases, with light obtained by the collision of

steel and flint, in the *steel-mill*, as it is called, a person could advance to any spot, empty a bottle of water there at any particular height from the floor, cork the bottle, and immerse the mouth of it, inverted in water. A portion of the air would thus be withdrawn, and at the bottom of the shaft, or at the mouth of the pit, it might be tried whether it were explosive or not. Even if it did not kindle, the degree of the approach to danger from the intermixture of a certain portion of the inflammable air, might be ascertained. That a mixture of carburetted hydrogen and atmospheric air should be capable of being inflamed, they must be in certain proportions to each other. Not less, as Dr Thomson has remarked, of the carburetted hydrogen than one-twelfth of the volume of common air must be present; and when it exceeds one-sixth of the volume of the air, it ceases to explode. All mixtures in the proportions between these will explode, but beyond these extremes will not. If the air withdrawn from the mine for examination does not explode, it may be discovered how near it is to the first proportion at which this will happen, by adding to different portions of it, certain proportions either of hydrogen or atmospheric air, until an explosive mixture is formed. If the addition of a small proportion of hydrogen, for example, were sufficient for this purpose, this would indicate the near approach to danger, by shewing that a very little farther intermixture of fire-damp would render the air in that part of the mine explosive. An excess of hydrogen, if it were present, would always be hazardous, for although it might not form properly speaking, an explosive mixture, still it would be inflammable. These experiments are so simple, that the more intelligent miners or superintendents might be easily taught to perform them. One of the most frequent causes of the unfortunate accidents that have occurred, seems to have been the want

want of any proper method to ascertain the extent of danger. In many of them the accumulation of the fire-damp was suspected; it was in trying to ascertain this that the explosion happened, and it is astonishing to observe, in some cases, the extreme imprudence with which this was done, by approaching with a common candle. In mines peculiarly liable to such accidents, it might be well to have a regular system of making such trials at stated periods. And this is more necessary, when it is considered, that all methods of lighting that may be proposed, are, strictly speaking, only calculated to lessen the danger from accidental firing of the gas; and that, in one point of view, they are a source of hazard, as giving the idea of greater security, and being liable, therefore, to lead to less strict attention to ventilation.

When the accumulation of gas to a dangerous extent is ascertained, it may be drawn off by various methods. A communication may be formed with a part of the mine in a state of thorough ventilation, and the rapidity of the current of air might be increased. Or, the foul air might be pumped out by a steam engine, or by an exhausting machine, such as that proposed by Mr Taylor *, brought to act on any particular part. (Note E.)

It is not necessary, however, to enter on the details of the system, which, with regard to several of the contrivances, are indeed sufficiently obvious, and which might farther be varied by local circumstances, and be improved by a knowledge of these, and by experience. My object has been merely to state the general method, and explain its principles, with any collateral observations which appeared to me to be of importance.

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* Thomson's Annals, vol. iii.; or Philosophical Magazine, vol. xxxviii.

It is farther obvious, that the mode of producing illumination by coal gas may be connected with this method, at least so far as regards the fixed lamps. The gas might be prepared by a furnace, at a part of the mine where there would be least hazard from carrying on the operation,—at the bottom of the air-shaft, for example, where a strong current of air moves outwards, and the gas might be conveyed through a main trunk, to pipes terminating by a small aperture within the glass-case. Its combustion, as it issued from the aperture, might be sustained with safety by the current of common air, supplied in the mode already described. A very brilliant illumination might thus be obtained; and from the peculiar advantages of situation, this might be done so economically, that it might render moveable lamps, to which the method could not well be applied, unnecessary. It might even become a source of advantage, by getting rid of much of the waste coal, and converting it into coak. It is possible, however, that the attention required in the process, and its interfering with the operations of the mine, might, independent even of any supposition of hazard, render the propriety of its introduction doubtful.

The general method, I may add, will prove equally effectual in obviating the danger from *Choak-damp*, carbonic acid gas, the other evil which miners dread, and which often also occurs in caverns, subterranean passages, and other situations. From its specific gravity being so much greater than that of atmospheric air, it is known to remain nearly at the floor, and to extend very gradually upwards. A person, therefore, may advance with safety, where it is present, by the precaution of holding

holding in the hand a candle within a glass case, having a tube attached to it supplying air from near the floor. The supply of air to support the combustion being thus from beneath, the presence of carbonic acid gas to any hazardous extent would soon be discovered, by the flame of the lamp becoming fainter, and being at length extinguished, while the respiration of the individual would not be affected: And by raising the open end of the tube to different heights from the ground, the extent to which the atmosphere of carbonic acid reached would be ascertained. By establishing in mines in which the choak-damp is liable to occur, a system of lighting similar to that which has been described, the danger from it would be effectually obviated.

NOTES.

NOTES.

Note A. page 2.—The production of fire-damp is much less considerable in the Scotch collieries than in those of the west, or the north of England. It would be important to discover the cause of this, but it is not very obvious. Probably it arises from the smaller scale on which they are wrought. In some of them, however, it does occur, though in quantities not so considerable, but that it is usually carried off by the common mode of ventillation, or by firing it as it begins to accumulate. In the mines in Ayrshire, it is the practice to fire it daily. Within these few years, explosions from it have in different cases been productive of fatal accidents, some of them, especially in the mines in West Lothian and Stirlingshire, to a considerable extent.

Note B. page 4.—No question can be more important in relation to the subject of the fire-damp of mines, than that with regard to the causes of its production. The facts stated in the text prove, that it is not entirely from the old wastes that the gas is discharged, though they may afford a large quantity of it. Its evolution might be considered as a circumstance in part connected with the original formation; the gas might be supposed to have been formed with the coal, to be confined by pressure in its mass, or its interstices, and to be liberated as the pressure is removed by the working. The density of the mass of coal, however, can scarcely be supposed to be such, as to have confined the gas from its first consolidation, and it must

must therefore rather be regarded as a new and continued production. There is no operation from which, under this point of view, it can be derived with so much probability as from the slow decomposition of water permeating the coal; and the connection of the production of carbonic acid with the carburetted hydrogen, seems to prove that this is its origin. That water transuding slowly through a mass of coal, and existing in it in some measure under pressure, will be decomposed, is, from the consideration of the general agency of water on carbonaceous substances, extremely probable. The evolution of the same gas from marshy situations, there is every reason to believe, depends on the decomposition of water by carbonaceous matter; and the occurrence, not unfrequent, of large masses of small coal accumulated at the mouths of the pits, and exposed to humidity, taking fire spontaneously after a certain time, can scarcely be ascribed to any other cause than to such a decomposition, and may therefore be regarded as a proof of it. There are circumstances, too, connected with the production of fire-damp which seem to prove that this is its origin. Thus it does not occur in all coal-mines; in some it is abundant, in others it is almost unknown; and this seems to be considerably dependent on the state of humidity in the coal. In the collieries in this country,—for example, fire-damp scarcely ever occurs in those of Mid Lothian; while in those of West Lothian, of Stirlingshire, Fife, and Ayrshire, it is not an unfrequent occurrence; sometimes to such an extent as to have been productive of considerable explosions, and in some of these mines its evolution is nearly constant, so that it is a regular practice to remove it by firing it. I have been able to discover no cause for this peculiarity, but the comparative state of dryness and humidity. It is not owing entirely to the depth, for this differs little; in some of the mines of Mid Lothian,

 the

the depth is 57 or 60 fathoms; in the Grange Colliery, in West Lothian, where an explosion happened some time ago, the depth is about 50 fathoms; and in the Ayrshire mines, the first bed of coal is at a depth of 30 fathoms; the second at a depth of 26 below this, not deeper, therefore, than that of the Mid Lothian collieries, where the gas does not occur; and farther, from the upper bed of the Ayrshire coal, fire-damp is given out as abundantly as from the lower. But the collieries of Mid Lothian are perfectly dry; the coal being what are called *edge seams*, that is, in strata vertical, or highly inclined, a disposition which allows the water to pass off more readily. In Ayrshire again, at Borrowstounness, and at Valleyfield, where there is the generation of fire-damp, I am informed there is much water, which seems even to percolate the coal. This is particularly the case in Ayrshire, the water dropping from the wall of coal, and a current or *blower* as it is called, of fire-damp sometimes escaping with water. The still greater production of fire-damp in the English mines, is probably owing to the much larger scale on which they are wrought, and to the deep and extensive workings being favourable to the collection of water. It accordingly appears, from the accidents which have repeatedly happened from water bursting into mines, that it is accumulated in old pits and excavations in immense quantities, and that it transudes through the mass of coal. The last accident which occurred, that at the Heaton Colliery, in which seventy-five individuals were destroyed from the bursting of water into the mine, is a melancholy proof of this. These causes, too, particularly the depth of the workings, favour the accumulation of the gas. This in some measure accounts for the accidents from explosion having become more frequent in these mines, notwithstanding the improvements in their ventilation, and gives some ground for the fear that its

accumulation

accumulation may still increase. The more numerous and extensive the excavations become, it is justly remarked, in a pamphlet published by the Literary and Philosophical Society of Newcastle, the greater will be the difficulty of guarding against surrounding wastes filled with water, or carburetted hydrogen, or carbonic acid gas; and when, at a future period, it shall be found necessary to work the lower seams in this coal-field, the operations of the miner must be carried on under immense accumulations of water. If these views be just, the propriety of impressing on the coal proprietors the necessity of conducting the workings on a better system than has hitherto been followed, will be obvious; and from the apparent indifference of many of them on this subject, the propriety of legislative interference to regulate the economy of the mines, which has been repeatedly suggested, will scarcely be questioned.

It is a curious circumstance, that in those mines in which the fire-damp does not occur, the production of choak-damp, or carbonic acid gas, is not unfrequent. Thus it often occurs in the Mid Lothian collieries, and sometimes at no great depth.

Note C. page 37.—It is been said, that the inflammable air sometimes issues from the floor of the mine, and this has been stated as a sufficient objection to the method I have proposed. The fact is, that it seldom comes from the floor, but usually from the sides of the wall of coal; and in general even the discharge is rather from nigh the roof than from beneath, as must indeed be the case in the escape of an elastic fluid from an imperfectly solid mass. But even if it did *issue* from the floor much more frequently than it does, it does not *remain* there, but rises to the roof, where the accumulation of it, and the mixture of it with the atmospheric air which renders it explo-

sive, uniformly take place, as all the facts connected with the state of the air in the mines prove; nor can any accumulation of it take place, which shall reach the floor of the mine, but by its filling the space from the roof downwards, mixed more or less with the atmospheric air. All that is necessary, therefore, is to guard against the chance, extremely small in itself, of the open end of the tube being in the direction of a stream of the gas, if at any time it should issue from the floor, and this is easily done by the methods stated in the descriptions, of the tube being turned up at its extremity, or of its being closed for the height of two or three inches, with apertures above this height, to admit the air. Any small quantity which might be brought by the current of air entering the tube must be unimportant, and any danger from this source must require such a combination of circumstances as may well be expected never to occur,—that of the tube being in the direction of the current of gas,—of the mixture of it with atmospheric air being in that limited proportion when it reaches the flame of the lamp in which it explodes, and of the whole air at the floor of the mine being also in that state in which it will explode; and all this independent of the circumstances, that by any such mixed air passing into the lantern, the flame of the lamp will be extinguished instead of explosion happening, and that explosion, even if it did occur, would not be conveyed along this length of tube.

The same arrangement, with regard to the tube, obviates another possible inconvenience,—that of the entrance of carbonic acid gas, which, from its greater specific gravity, may sometimes occupy the floor of the mine. It seems scarcely ever to be accumulated to this extent in mines in which fire-damp is generated; and if it were, its entrance into the lantern, would be productive of no other accident than that of extinguishing

extinguishing the flame. But even this is easily obviated, by admitting the air at any height from the floor which may be found requisite.

Note D. page 42.—To the observations in the text, on the construction of the lamp, a few details may be added.

The principal circumstance requiring adjustment, is that of the size of the aperture by which the heated air escapes. If it is not sufficiently wide, the flame is faint, and on the movement of the lamp becomes unsteady, and is liable to be extinguished. If it be too wide, there may be some risk of a current of air entering the lantern by it, especially if the under tube is not sufficiently wide, by which the whole security from the method would be lost. The due size is most easily found, by affixing to the aperture at the top a conical tube, and cutting this down in successive trials, until the diameter is attained at which the flame is steady and bright. It is not easy to give a precise dimension, as the flame is dependent on the breadth and height of the wick, the purity of the oil, and the state of the air; but I find that in a lantern of the size of the moveable one mentioned in the text,—five inches in height by three in width, with a flat cotton-wick three-tenths of an inch broad, and burning so as to consume about two ounces of oil in six hours, the diameter of the aperture being a very little less than half an inch, admitted of the flame being steady and bright when the combustion was fully established. In the mine it may be required to be a little larger. When the lamp is kindled, it remains for a minute or two more faint, and if moved hastily in this state, is liable to be extinguished. One aperture is preferable to two or three smaller apertures, as there is less risk of any counter-current; and to guard also against this in any movement of the lamp, it is proper to have the opening

opening in the form of a short tube. The heat of the air issuing from an aperture of this size, with a flame such as has been described, I found to be 370°, the bulb of the thermometer being in the current exactly at the orifice; when introduced entirely within the aperture, and immediately above the flame, it rose to 465°. Air of this temperature, it is obvious, cannot inflame fire-damp, or any mixture of it.

No very accurate adjustment is required with regard to the size of the tube conveying the air into the lantern. It is sufficient to have it wide enough; the state of the flame is then regulated entirely by the size of the upper aperture, and any slight excess of width in the tube beneath is of no importance. I find that with a lantern and lamp of the above size, a tube three-fourths of an inch in diameter interior measure, and three feet and a half in length, answers very well. This circumstance, of no accurate adjustment being necessary, gives an advantage to this method in actual use. Where the safety of the lamp depends on such an adjustment, it is difficult to construct it in such a manner that it shall burn with a bright flame, and steadily, so as not to be liable to be extinguished by movement, or by the least failure either in the current of air, or in its purity. The bringing the air from the floor renders any such adjustment unnecessary, and allows, therefore, of a more bright and steady flame being produced with entire safety. And by the lamp being thus always supplied with the purest part of the air, it will continue to burn where any safety-lamp on a different principle must be extinguished, and of course will enable the miner to work in situations where no other will be of any use.

In fixed lamps, the length of the tube must be regulated by the height of the passages. The thickness of the two beds of coal at Newcastle is about six feet each. But it is unequal, and

and in some places is not more than four feet. On the other hand, the occasional falling of the roof forms dome-like cavities above this height; it is in these that the inflammable air chiefly accumulates, and in the lower passages or workings, where they are open, the draught of air must in general prevent it from being collected. Hence the limits to the elevation of the lamp, and of course to the operation of the principle on which the method is founded, are less than they at first appear; though even the height of three, or three feet and a half from the floor, while it is probably best adapted to the necessary illumination, will give the requisite security.

A lamp with oil is more convenient than a candle, as requiring no adjustment with regard to the wick; and by the common contrivance of a plate with a screw on the aperture, the oil is prevented from being spilt, on any occasional inclination of the lamp. The usual time of a miner's work is six hours; the lamp, of the size just now mentioned, with fresh oil and wick, burns seven hours. The miner, therefore, may take it with him newly trimmed, and the lantern need never be opened in the mine, by which any risk from the communication of its flame to the surrounding air may be avoided. If it were necessary that it should burn for a longer period, it might, without any inconvenience, be made of a larger size; and the wick might be made so as to admit of being raised by a contrivance similar to that of Argand's. All the joinings of the case or lanthorn, it is obvious, ought to be as close as possible.

By employing a metallic lantern, with a lens of very thick glass in front, the risk from breaking, which is incurred when a glass case is used, is avoided. This construction has other advantages. It affords a great deal of light in the most favourable manner; the illumination being directed with less loss on the space where it is required. Where the situation admits of it,

it, a lantern of this kind can be made to project light in a straight line to a great extent, and illumination may be thrown into a passage, or along the side of a wall which is to be worked, by its being placed at one extremity. This may not only have the advantage of economy, but of greater safety, so as to render this method proper to be employed on this account alone, independent of any other consideration; for the lamp being placed at some distance from the miner, the risk is avoided from the concussion arising from his working, from any fall from the roof, or from the sudden discharge of the inflammable gas from any opening in the coal. Illumination may also be thus obtained, in situations where, from imperfect ventilation, it is difficult to support the combustion of a lamp, such as the close extremities of passages or of new workings. These are the very places, too, which are more peculiarly liable to the accumulation of the inflammable air; and in both respects, therefore, the advantage is obvious, of a mode by which, where the direction of the working admits of it, (which it will almost always do to a certain extent,) light may be thrown from a distant spot, where the same difficulty and the same hazard do not exist. Lastly, In ascertaining the state of the air in passages where danger is suspected, or in exploring them after the accident of an explosion, the same method will give greater safety. Where a more diffused light is wanted, this is easily attained by the surface of the lens being more or less scratched.

Note E. page 45.—When the workings of a mine are carried to a considerable distance from the course of the current of air, without a corresponding shaft being made, the ventilation becomes very imperfect. In this case, it is stated in a very candid communication by Mr Scott, (*Edinburgh Journal*, December

December 1815,) that the lamps burn with more brightness near the floor than near the roof of the mine; the lamp, therefore, with the tube, will be adapted to such situations, or at least the circumstance of the air being brought from the floor will counterbalance any obstacle from its being inclosed, and will allow it to burn as well as an exposed lamp would. In the Scotch coal-mines in general, the system of ventilation appears to be even less perfect than it is in the mines in the north of England, probably from the same necessity not having existed for rendering it equally perfect, as the production of fire-damp is so much less abundant. The circulation of the air in them is often so languid, that a method of lighting by a close lamp would perhaps be attended with difficulty, at least if it were necessary to place the lamp in such situations, which, however, the contrivance of employing a lantern with a lens, in some measure would obviate. The English mines present the combination of a more perfect ventilation with the constant production of enormous quantities of fire-damp; the object, therefore, is to guard against explosions from the accidental accumulation of the gas, while, at the same time, the general plan of ventilation admits of this being more easily carried into effect. A singular method, which shews both the imperfect ventilation, and the moderate extent within which the gas is generated in the Scotch mines, has been practised,—that of firing the quantity accumulated at a stated period,—in some mines daily. A miner enters the mine with a lamp inclosed in a lantern; he advances as far as is proper, holding it as low as possible, and lying down on the ground, or sometimes even in a trench dug in the ground; he removes the lamp from the lantern, and raising it, or advancing it towards the closed extremity of the working, where the gas is collected, fires it. Mr Scott, in the communication referred to, proposes a plan of

 firing

firing the gas as it collects, by a lamp of a particular construction, suspended nigh the roof. This would be safer than the old method, by which the workmen are sometimes injured, and which, whenever the production of the gas becomes considerable, is evidently impracticable. But the exhausting machine noticed in the text, (page 45.) may always be applied with sufficient effect, and where the gas is slowly collected, must be preferable to any other method, where a current of air cannot be completely established. In the Hurlet mine, near Paisley, it has been employed with entire success, and on so small a scale, that it is worked with a hand pump. The cylinder exhausting the air is 23 inches in diameter, and it makes a 13 inch stroke 13 times *per* minute; it discharges, therefore, in that time, 40 cubic feet of air. It is worked by a boy, and only as it is required. Tubes of tinned iron are connected with it, which are prolonged as the excavation extends.

Description of the Figures. (Page 36.)

Fig. 1. Plate 1. represents the Fixed Lamp.

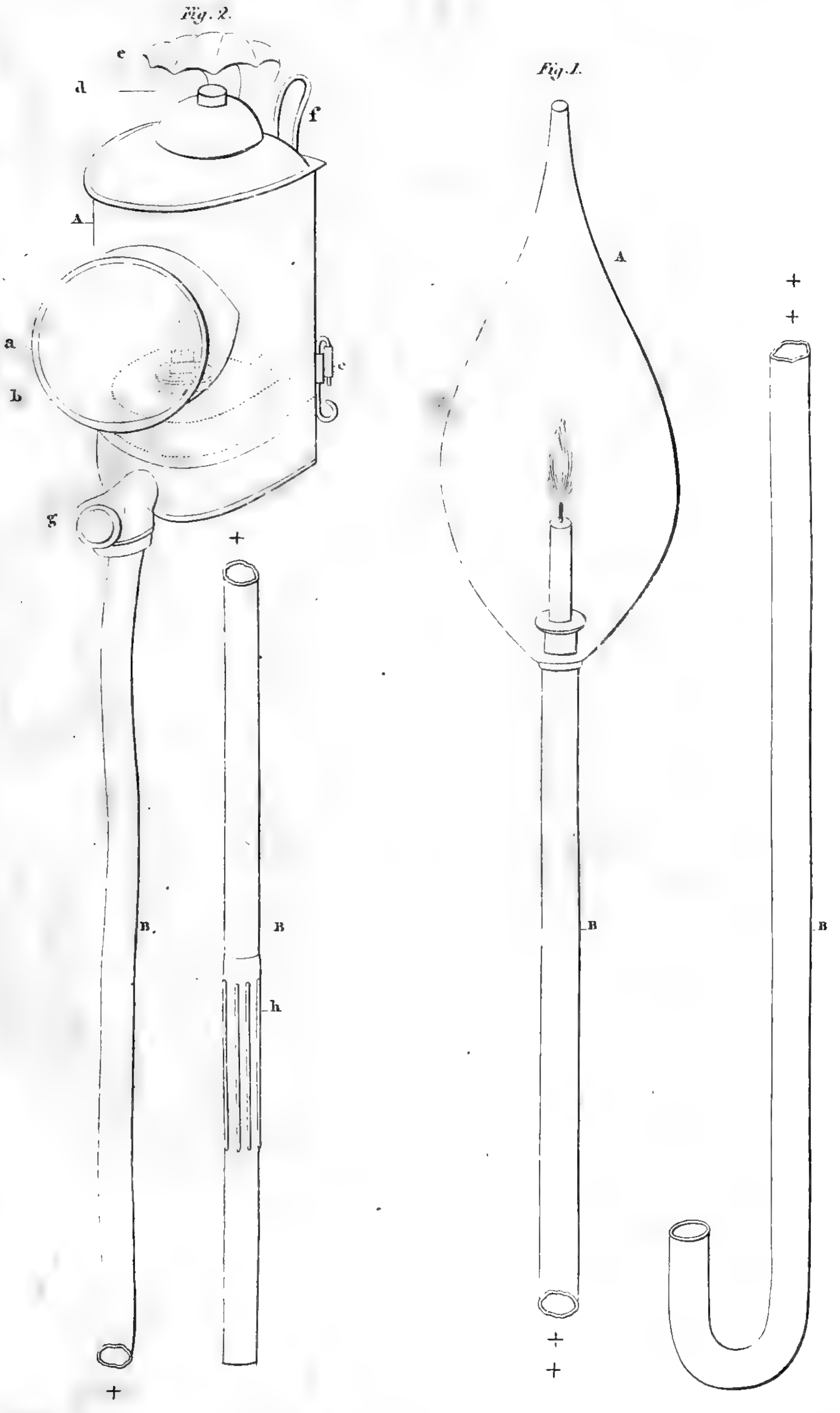
A, The glass case within which the candle or lamp is placed in a socket, with the aperture at the top, of a sufficient size to admit of the escape of the smoke and heated air.

B, The tube of tinned iron, or of copper, which enters beneath the socket, conveying air from the floor to support the flame. To shew the length, it is represented in two parts, and at the under extremity it is turned up to the height of three inches, to attain the advantages explained, p. 52.

Fig.

PLATE I.

Engraved for the Royal Society Trans. Vol 8 page 58.



Engraved by W. & D. Lizars Edin.

Fig. 2. represents the Moveable Lamp.

A, Is a lantern of tinned iron, five inches in height, and three inches in width, with a glass lens *a*, three inches in diameter, projecting half an inch in front. The lamp *b* is introduced at an opening behind, which is closed with a cover secured by a wire, passing into a small groove or tube, as represented at *c*; *d* is the aperture in the small dome at the top, by which the smoke and air escape; *e* is a small projecting plate to disperse the current of hot air; *f* the handle, which rises from the double back.

B, Is the tube of leather, with a spiral wire within, to prevent its compression, which conveys the air to support the flame; three-fourths of an inch in diameter, and from three to four feet long, and represented in two parts, to shew its length. It is adapted by a screw to the short projecting tube *g*, at the bottom of the lantern; and the lamp within resting on a plate at the height of half an inch from the bottom, the air enters beneath this, and rises by its sides. To the under end a tin tube *h* is adapted, closed at the end, with apertures in the sides, at the height of about three inches, to admit the air; this having the same advantages as the turning up of the tube in the former figure, and in a moveable lamp being more convenient.

IV. *On the Lines that divide each semidiurnal Arc into Six equal Parts. By* W. A. Cadell, Esq. F. R. S. Lond. & Edin.

[*Read 3d June* 1816.]

THE divisions of the day which different nations have employed, are denoted by hour-lines of various kinds on the sphere. Of these hour-lines, drawn on a supposition that neglects the inequalities of the Earth's motion, there are three kinds.

The first kind denotes *hours counted from the meridian, equal to each other at all declinations of the sun.* These lines are *great circles* on the sphere, passing through the poles of the equator, and every pair intercepting a *similar arc* on each of the parallels. Of this kind are the hour-lines of sidereal time, counted from the meridian, and the hour-lines of solar time, counted from the meridian.

The second kind of hour-line denotes *hours counted from the horizon, equal to each other in duration at all declinations of the sun.* These lines *are great circles* which touch the greatest visible parallel on the one hand, and the greatest invisible parallel on the other; each pair of these great circles cuts off a *similar arc* from each diurnal arc. They are the horizons of different

different points of the parallel which passes through the zenith. The Italian hours counted from sunset, and the Babylonian hours counted from sunrise, are denoted by lines of this kind.

The third kind of hour-line, and of which it is proposed to speak more particularly here, denotes *hours varying in length as the declination of the sun varies*, each hour being one-sixth part of the semidiurnal arc, whether that arc be a smaller portion of the circumference, as in winter, or a greater, as it is in summer. On the oblique sphere these lines are *not great circles*, and each adjacent pair intercepts *a dissimilar arc* on each semidiurnal arc. This kind comprehends the hour-lines of the ancient Greeks and Romans, which denote hours called *hectemoria* *, that is, sixth parts of the semidiurnal arc.

The curvature of these lines is visible when they are drawn on a globe; it is likewise seen in their gnomonic projection, in the following manner.

Figure 1st is a perspective view of the lines which intercept one-sixth part of each semidiurnal arc; the point of sight is the centre of the sphere; the plane of projection touches the sphere at the pole of the equator, and is therefore parallel to the equator; the latitude is 66° 30′; at this latitude the whole of each hectemorial hour-line is gone over by the sun in a year. This perspective view is the same as the central or gnomonic projection of the sphere on the inside of a plane which touches the sphere at the pole of the equator; it forms an inferior equinoxial dial for the latitude 66° 30′, when placed parallel to the equator, with its inscribed surface downwards, and the point xxiv. elevated.

In

* Ἑκτημόριον, sexta pars, sextarius, is used by Ptolomy. The lines that separate the hectemoria from each other are in this paper called *hectemorial lines*.

In this view the projections of the parallels are circles, and equal arcs of the parallels are represented by equal arcs of their projections: the construction of the lines which intercept one-sixth part of each semidiurnal arc, is therefore performed by dividing the projection of each semidiurnal arc into six equal parts, and connecting each point of division with its corresponding points on the projections of the other semidiurnal arcs.

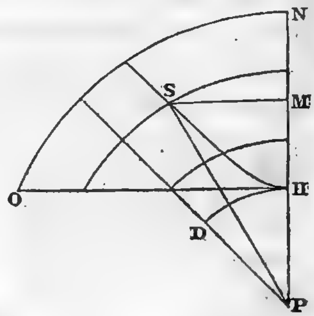
In figure 1st, these hectemorial hour-lines are seen to converge at that point of the meridian which is marked 66° 30′. This is the point of contact of the horizon, and greatest unseen parallel; it is also the point where the mid-day part of the meridian cuts the horizon. At this point the semidiurnal arc is indefinitely small, and therefore the lines which divide it into six parts must be indefinitely near to each other, or, in other words, must converge.

At this point of convergence each hectemorial line is inclined at a considerable angle to the meridian. As the line proceeds, the inclination becomes less, till it is nearly as small as the inclination of the astronomical hour-line, which this hectemorial line cuts at the equator; and the hectemorial line on this projection is asymptotic with that astronomical hour-line. For the distance of their intersection from P, measured on the plane of projection, is infinite, being equal to the distance of the intersection of the equator and plane of projection, two planes parallel to each other; and however far the projection is extended, the two lines approach indefinitely, but do not meet.

Take, for example, the third hectemorial line HS (in the figure on the margin) which cuts the ninth astronomical hour-line at the equator; the projection of a great circle which intersects the ninth astronomical hour-line at the equator, is a straight

straight line parallel to the ninth astronomical hour-line on this projection; but the third hectemorial line is continually approaching to the ninth astronomical hour-line; the distance between them at the horizon being HD = *arc.* $45^0 \times$ *tan. polar distance,* and afterwards it is (*arc* $45^0 - \frac{1}{2}$ *semidiurn. arc*) $\times$ *tan. polar distance of the star*; $\frac{1}{2}$ *semid. arc* increases, but never attains to be **45°**, so that the distance never becomes equal to nothing, and *tan. polar distance* increases indefinitely.

All great circles are seen under the form of straight lines in this projection of the sphere; and therefore the projection of one great circle cannot be an asymptot to the projection of another; it follows, that the projections of the hectemorial lines are not projections of great circles. If a straight line be drawn through the point H, (in the figure on the margin,) cutting off a given aliquot, one-half, for example, from a semi-diurnal arc on the projection, it will cut off a smaller aliquot from the meridional extremity of the other semidiurnal arcs, in proportion as they are nearer to the point H; and in order that a straight line drawn from H may cut off the same aliquot part from several concentric arcs which are included between the versed sine HN and sine HO of the outer arc, it is necessary that the chords of these arcs be parallel to each other; which happens only in the case where all the arcs are of 90°, then H coincides with P, and then the straight line which cuts off the same aliquot from every arc, is a line passing through P the centre; and this sole case is a central projection of a sphere so placed, that each semidiurnal arc is 90°, the poles of the equator being in the horizon. In this position alone are the



hectemorial

hectemorial lines great circles, and in this case they coincide with the astronomical hour-lines, and pass through the poles of the equator. When the poles are thus in the horizon, all the three kinds of hour-lines coincide. When the pole is in the zenith, the second kind or horizontal, and the third or hectemorial hour-lines cease to be, because then the horizon does not cut any of the parallels into diurnal and nocturnal arcs.

To express algebraically some of the above-mentioned properties. Let the abscissæ x be taken on the meridian HM, and the ordinates y at right angles to the meridian, the central projection gives

$$y = \sin.\ \frac{n}{6}\, s\ \tan.\ \textit{polar dist. star},$$

$$x = \left\{ \cos.\ \frac{n}{6}\, s - \cos.\ s \right\} \tan.\ \textit{pol. dist. star}.$$

PH is $\cos.\ s$; the cosine of the semidiurnal arc. PM is $\cos.\ \frac{n}{6} s$; the cosine of the fractional part of the semidiurnal arc. MS is $\sin.\ \frac{n}{6} s$; the sine of the fractional part of the semidiurnal arc. PS is *tan. pol. dist. star*; the tangent of the polar distance of the star. n is one of the numbers 1, 2, 3, 4, 5; it is 5 for the curve that contains the first and eleventh hectemorial line; 4 for the second and tenth, and so forth.

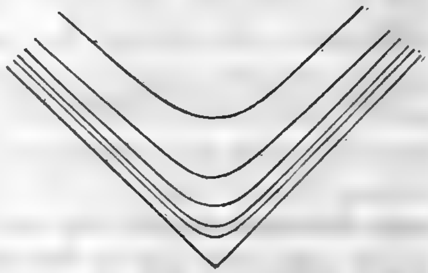
$\frac{y}{x} = \frac{\sin.\ \frac{n}{6}}{\cos.\ \frac{n}{6} s - \cos.\ s}$ is not the equation of a straight line, except in the case where $\cos.\ s = 0$, that is, when the semidiurnal arcs are all 90°, the poles being in the horizon.

$y =$

$y = sin. \frac{n}{6} s$ *tan. pol. dist.* in this equation, when the describing diameter arrives at H, $sin. \frac{n}{6} s = 0$, that is, the curve cuts the axis of the abscissæ at H, and when the describing diameter passes onward, the sign of $\frac{n}{6} s$ is changed, expressing that there are two similar and equal branches, one on each side of the axis of the abscissæ.

$Cos. \frac{n}{6} s$ has only one value, because $\frac{n}{6} s$ is never so great as 90°, and its cosine, therefore, does not become $= 0$, which it must do before its sign changes.

If a central projection of any one forenoon hectemorial hour-line, and of the afternoon hectemorial line equi-distant from the meridian, be drawn for different heights of the pole, it will be seen that these two hectemorial lines form for each height of the pole an equicrural curvilinear branch, including within it the corresponding hectemorial lines for a higher latitude, and all of them included within two astronomical hour-lines as asymptotes. With these asymptotes the pair of hectemorial lines coincides, when the poles are in the horizon. The figure in the margin represents the equicrural curves formed by the third hectemorial hour-line and by the ninth, which is the afternoon branch equi-distant from the meridian.



The curved branches are drawn for every ten degrees of latitude from 30 to 70 inclusive; the rectilinear asymptotes within

in which the curves are contained, are the ninth and fifteenth astronomical hour-line counted from midnight.

From figure 1st it may be collected, that the asymptotes of each hectemorial line are as follows:

The equicrural curvilinear branch, composed of the hectemorial lines	*has for asymptotes the astronomical hour-lines, counted from midnight,*	*which comprehend an angle of*
First A, and Eleventh IA,	VII. and XVII.	150°
Second B, and tenth I,	VIII. and XVI.	120°
Third Γ, and ninth Θ,	IX. and XV.	90°
Fourth Δ, and eighth H,	X. and XIV.	60°
Fifth, E, and seventh Z,	XI. and XIII.	30°

Figure 1st represents the portions of the day hectemorial lines which touch the greatest invisible parallel for the latitude 66° 30′; these are the winter portions. In order to delineate the portions which touch the greatest wholly visible parallel, or the summer portions, the projection is made on the plane which touches the sphere at the depressed pole; this projection is exhibited in figure 2d, which contains portions of curves, each of which is similar and equal to the curve of the same hour in figure 1st, but differently placed. It forms a superior equinoxial dial for 66° 30′, when placed parallel to the equator, with the inscribed surface upwards, and the point xxiv. elevated.

However extended the plane of projection touching the sphere at the pole be, still it will not contain the portions of the hectemorial lines that are near the equator. To have these, the plane of projection is taken at right angles to the equator, and touching the sphere at the depressed intersection of the

 equator

equator and meridian. This projection, which is drawn at figure 3d, contains some of the hectemorial lines from the point of their contact with the greatest wholly unseen to the points of their contact with the greatest wholly seen parallel. It forms a polar dial for the latitude 66° 30′, when placed parallel to the plane of the sixth astronomical hour-line.

The lines thus drawn in the above projections are curved; consequently they are not the projections of great circles; neither are they the projections of small circles, for if they were, they must necessarily touch the horizon at its intersection with the mid-day portion of the meridian, because in that point the hectemorial lines converge, and do not go on the other side of the horizon. Now, if small circles so placed, be drawn on the sphere or projected on a plane, it will be found that their course deviates entirely from the course of the lines bounding the hectemoria. The hectemorial lines, therefore, do not coincide with small circles on the sphere, nor with conic sections on the central projection.

The projections above exhibited shew that each pair of hectemorial lines for a given meridian and latitude, is an equicrural curved line; but this is only one branch of an entire curve, because the diameter whose extremity has traced a pair of these lines on the surface of the sphere, has still to complete its revolution, which is done when it has arrived by progressive and continuous motion at the point from which it set out. In order to accomplish this with the same kind of motion with which they described the first branch, the extremities of the diameter leave the two parallels that touch the horizon, and proceed to cut off the same aliquot part from the semidiurnal arcs belonging to this second point of departure, as they had done from the semidiurnal arcs of the first point of departure; the second point of departure is to be considered as the mid-day point of a horizon on the opposite side of the equator to the

the first. The extremities of the diameter, after having in this way passed several times to and fro between the greatest wholly unseen and the greatest wholly seen parallel, and after having completed one or more circumferences, attain precisely the two opposite points from which they set out, having formed two opposite re-entering curves on the surface of the sphere, and afterwards, in every subsequent revolution, the extremities of the diameter only retrace the lines they had described in their first revolution. The nature of the motion is such, that the describing points cannot go beyond the two parallels which touch the horizon, because there are no semidiurnal arcs beyond these parallels, and the constitution of the hectemorial lines, consists in cutting the semidiurnal arcs.

Whilst the extremities of the diameter describe two re-entering curves on the surface of the sphere, the diameter itself describes two opposite re-entering curved surfaces, whose common vertex is the centre of the sphere. These two opposite surfaces coincide with a straight line directed to the vertex, but do not coincide with a straight line in any other direction. In this respect they resemble a conical surface, and they may be considered as a kind of opposite cones with an undulated surface, included between the outer surfaces of the two opposite circular based cones that touch the horizon; the apex of each undulation being applied alternately to the one and to the other of these right circular based cones, so that as the common vertex of all these cones is the centre of the sphere, each of the two opposite undulated cones has two similar and equal bases or right sections, the one above the centre of the sphere, the other below it. Each of these bases is an uninclosed curve, composed of many equicrural branches, each equicrural branch having for asymptotes two straight lines that intersect in the axis; these two right sections are indefinitely extended; and the

the bicrural branches of one right section are alternately placed in respect to the branches of the other right section.

The undulated cone belonging to each of the five hectemorial lines is different from that of the others. The five upper bases are represented in figures 4th, 5th, 6th, 7th, and 8th, drawn according to the rules of the central projection, on a plane parallel to the equator, for the latitude 66° 30′. The full lines are the upper base of one of the two opposite undulated cones; the dotted lines are the upper base of the other opposite undulated cone. These bases or right sections are each made up of a pair of hectemorial lines for a given latitude, forming one equicrural branch, and of similar and equal pairs for other points of the same parallel of latitude and of the opposite parallel.

In order to present to the eye the image of one of the undulated cones, figure 9th is drawn. It is a shaded view of one of the two opposite conical surfaces to which the 3d and 9th hectemorial lines belong, for the latitude 66° 30′. The point of sight is in the plane of the equator; the distance is two diameters from the centre of the sphere.

The sections of the undulated cones, by a cylindrical surface circumscribed round the equator, are made up of the same branches; they are complete and re-entering, whereas the sections by a plane are always incomplete. These cylindrical sections are laid down in figures 10th, 11th, 12th, 13th, and 14th. The cylindrical surface is here unrolled; when restored to its cylindrical form, the inscribed curve is re-entering, and without a break.

Where the number of degrees between the upper apices of each undulation is a divisor of 360, the undulated cone is completed in one circumference, because at the beginning of the second circumference, the generating diameter enters into the path

path it had described in the first; this is the case with the three undulated cones which contain the third, fourth and fifth hectemorial lines, figures 6th and 12th, 7th and 13th, 8th and 14th.

The number of degrees between the meridians of two adjacent upper apices, is equal to twice the distance between the two adjacent sides of each undulation measured on the equator, or to four times the distance of the equatorial point of the hectemorial line from the meridian measured on the equator.

Where this number of degrees between the meridians of the upper apices is not a divisor of 360, but of a multiple of 360, the revolution is completed in as many circumferences as there are units in that multiple; because the generating diameter does not enter into its former path till it has described that number of circumferences. The undulated cone which contains the second and tenth hectemorial lines, having an interval of 16 equinoxial hours, or 240° between the adjacent upper apices; the describing diameter completes its revolution, and comes to the point from which it set out at the end of two circumferences or 720°; the number of the upper apices being 3, which, multiplied by 240, is equal to 360×2; this is seen in figures 5th and 11th.

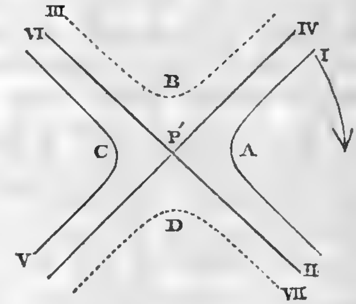
The undulated cone which contains the first and eleventh hectemorial lines, figures 4th and 10th, having an interval of 20 equinoxial hours, or 300° between the upper apices of the undulations, the describing diameter must go over five circumferences before it comes to the point from which it set out, because 300 multiplied by 6, which is the number of upper apices, is equal to 360×5.

The

The circumstances of each of the undulated cones with respect to the number of undulations and of circumferences, are as follows:

The undulated cone containing the hectemorial lines	*intercepts between two adjacent upper apices,*		*The number of upper apices is*	*The number of circumferences in which a revolution is completed, is,*
	In degrees.	*In equinoctial hours.*		
First A, and eleventh IA,	300	20	6	5
Second B, and tenth I,	240	16	3	2
Third Γ, and ninth Θ,	180	12	2	1
Fourth Δ, and eighth H,	120	8	3	1
Fifth E, and seventh Z,	60	4	6	1

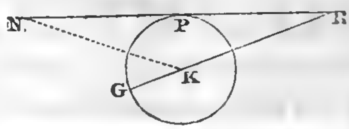
Four of the five undulated cones have opposite and similar undulated cones, formed by the remote half of the generating diameter; but in that undulated cone which contains the fourth hectemorial line Δ, and eighth H, the two opposite undulated cones coincide in one, as is seen in figures 7th and 13th.



The algebraic formula expresses the different branches of the right section of the undulated cone, by the change of sign of *tan. pol. dist.* in the values of x and y;

$$x = \left(\cos\frac{n}{6}s - \cos s\right) \textit{ tan. pol. dist.}$$

$$y = \sin.\ \frac{n}{6} s \textit{ tan. pol. dist.}$$



For example, in the right section of the undulated cone which contains the third and ninth hectemorial

temorial lines, the radii from the centre P′ are = *tan. pol. dist.* and the co-ordinates are affected by that quantity. If the generating diameter GKR move in the direction of the dart, and set out from I; it describes the curvilinear branch A, and *tan. pol. dist.* is positive till the generating diameter come to the situation II, where it is infinite. If its revolution be continued, it re-appears at III, on the other side of the centre with the negative sign, and gives the curvilinear branch B; at IV. it is again infinite, and re-appears positive at V, going on to form the curvilinear branch C; at VI. it is infinite; and at VII. it becomes negative, and forms the branch D; it then comes out positive at I, and goes over its former path. The way that the change of sign takes place is apparent, by considering that whilst the diameter is moving in the direction of the dart, it has another motion at right angles to the plane of projection. Let GPK be a section of the generating sphere, at right angles to the plane of projection; NPR being the section of that plane; GKR is the generating diameter; and when in the position GKR, then *tan. pol. dist.* = PR, and is positive; when GK is parallel to PR, then *tan. pol. dist.* is infinite. If the motion in this plane be continued, *tan. pol. dist.* passes to the other side of P, as at PN, and is affected with the contrary sign.

Most of the writers who have spoken of the hectemorial lines, have treated them as great circles, because their intertropical parts, at a moderate height of the pole, coincide sensibly with great circles; and it is this case with which authors had to do in considering the gnomonic projection of the hectemorial lines for the climates of Greece or of Italy. The writings of a considerable number of authors on this subject have been consulted, and they all take the hectemorial lines for great circles, except CLAVIUS and MONTUCLA. CLAVIUS demonstrates that the antique hour-lines do not coincide with great circles;

and

and MONTUCLA merely states, but without discussion, that they are curves of a peculiar nature *.

It has been shewn above, chiefly by means of a projection on a plane touching the sphere at the pole, that the hectemorial hour-lines on the oblique sphere are not great circles; and because the describing diameter, in order to form a continuous and uniform surface, must go on moving during its whole revolution with that motion which it had in the beginning of its course, and must be always included between the two parallels that touch the horizon, it is concluded that the curved surface whose

* The passages from CLAVIUS and MONTUCLA are as follows:

CLAVII *Astrolabium* lib. i. lemma 39. "Circuli maximi transeuntes per horas inæquales Æquatoris, et duorum parallelorum oppositorum, non necessario per horas inæquales parallelorum intermediorum transeunt in sphæra obliqua." He gives a demonstration of this, and concludes, in the *scholium*, that in order to delineate the antique hours with strict accuracy, a considerable number of the semidiurnal arcs are to be divided into six parts, and the corresponding points of division joined.

MONTUCLA, Hist. des Math. tom i. edition de 1758: "Les lignes de ces sortes d'heures [les heures antiques] ne sont point droites comme les precedentes, mais courbes, et meme d'une forme tres bizarre; de sorte qu'on ne peut les decrire qu'en determinant plusieurs points de chacune; la manière de les trouver se presentera facilement à tout geometre; c'est pourquoi nous ne nous y arretons pas."

The circumstance mentioned in the beginning of the paragraph to which this note refers, has led the celebrated and profound astronomer DELAMBRE to controvert the opinion of MONTUCLA in the following words: "MONTUCLA dit, en parlant des heures temporaires antiques, qu'elles sont courbes, et même d'une forme très bizarre, &c. *Hist. des Mathem.* tom i. On ne conçoit pas comment une pareille inadvertance a pu echapper à un homme aussi instruit; car si la surface est spherique, ces lignes seront des grands cercles; et si la surface est plane, elles seront des lignes droites, puisqu'elles seront les intersections des plans de ces grands cercles avec le plan du cadran." DELAMBRE *sur un cadran antique trouvé dans l'isle de Delos, et par occasion de la gnomonique des anciens; notice lue à la classe des Sciences Physiques et Mathematiques de l'Institut Royal de France, le* 10 *Octobre* 1814.

whose section contains the hectemorial line, is a kind of undulated conical surface; and the right sections of the surface are two infinite unenclosed bases, one on each side of the vertex of the undulated cone. This section, at right angles to the axis, consists of several bicrural branches, varying in number as the cone belongs to each different hectemorial line.

Of the Gnomonic Instruments of the Ancients.

The object of the preceding pages has been, to treat of the curves to which the hectemorial lines belong. As an appendix, it may not be improper to enumerate some of the remains of art which contain the antique hour-lines; for these hour-lines are the intertropical parts of the hectemorial lines. Several examples of these gnomonic instruments exist.

The first to be mentioned, and the most perfect, are the eight sun-dials on the Tower of Andronicus Cyrrhestes, at Athens. They appear to have been coeval with the building, and to have formed part of the original design, as may be inferred from the care with which they are delineated, and from the greatest part of the surface of the wall being left plane to receive the lines. This tower is mentioned under the denomination of *horologium* by Varro, who flourished in the 85th year before the Christian era; it is also spoken of by Vitruvius. The carefully wrought channels, and cylindrical cavities in the pavement, and the cylindrical chamber at the south side, have led to the conjecture, that, besides serving to shew the hour when the sun was shining on it, the tower was formed to contain some machine of the nature of the clepsydra, whereby the hour might be known at all times for the use of the city. Another of the destinations of this tower was to indicate the

direction of the wind. Each of the eight dials is exposed to one of the eight principal equidistant points of the horizon; two of the dials being parallel to the plane of the meridian. The radii of the spheres from which the dials are projected, vary; the smallest being about eight inches, and the greatest about twenty-five inches. In STUART'S *Antiquities of Athens*, the building and the dials are represented in detail.

The second example of the gnomonic lines of the ancients, is in the valuable collection of antiquities brought from Athens by the Earl of ELGIN. It consists of four vertical dials, two of which are nearly south-east, and two nearly south-west in azimuth. They are inscribed on a block of white marble, which bears the maker's name. The radius of the generating sphere is about four inches and a half.

The third example, is a projection of the antique hour-lines on the inner surface of a cone whose axis is parallel to the axis of the earth. It exists at Athens, and is figured by STUART.

The fourth is a small east dial, on a vertical plane, described by DELAMBRE. It was found at Delos. The radius is half an inch.

The fifth is a piece of Roman workmanship, figured by BOISSARD *. It is composed of five dials on the upper part of a squared block of marble; three of the vertical sides of which are covered with an ancient Roman agricultural calendar. In like manner, the treatise of PALLADIUS *de Re rustica*, which is a set of agricultural directions for every month, contains a gnomonic table, shewing the length of a man's shadow for each month, and for a climate in Italy.

Some

* BOISSARDI Antiquitates Romanæ.

Some fragments of ancient dials are also published by GRÆVIUS *.

Except the table of PALLADIUS, the instruments above mentioned are each made for an unvarying azimuth. In the description of the antiquities of Herculaneum †, there is represented and explained a dial whose azimuth is changeable, to suit the hour and the different declinations of the sun. It is drawn on an irregularly curved surface of bronze, and the declinations are marked with the Roman names of the months.

The ancient names of different kinds of fixed and moveable sun-dials, with the names of their inventors, are given by VITRUVIUS.

Something respecting the time of the first introduction of gnomonic instruments, is to be collected from Greek and Roman authors. The ancient inhabitants of Egypt appear to have cultivated astronomy at a time prior to the earliest historical accounts; and they have left a monument of their practical skill in that science, in the accurate meridional position of these most ancient of human works the Pyramids. Portions of their knowledge were diffused amongst the Hebrews and Babylonians.

The astronomical science of the Greeks was derived partly from the Egyptians, and partly from the Babylonians ‡. THALES ‖ acquired his knowledge of astronomy and geometry from the Egyptian priests, and introduced these sciences into Greece. The gnomonic projection of the sphere is a branch of the doctrine of spherical astronomy, and when applied to the

* GRÆVII Thesaurus Antiquitatum Romanarum.

† Le Pitture antiche d'Erculano, tom. iii. Napoli 1762.

‡ HERODOTUS.

‖ DIOGENES LAERTIUS.

the purpose of shewing the parts of the day, constitutes the Sun-dial, an instrument which may be supposed to have come into use soon after the introduction of astronomy. According to DIOGENES LAERTIUS and PLINY, the first gnomonic instrument that appeared in Greece was constructed by a disciple of THALES, about the 545th year before the Christian era, and 115 years before the death of PERICLES. SAUMAISE * contends, that this instrument was for the use of astronomers only, and that sun-dials did not come into general use in Greece till 200 years after, that is, a short time before the age of ALEXANDER.

The Romans got their first sun-dial from one of the Greek cities of Sicily †, 260 years before the Christian era; and for nearly a hundred years after, they possessed no artist acquainted with the principles of the instrument, so as to construct one adjusted to the climate of Rome.

* SALMASII Plinianæ Exercitationes.

† PLINII Hist. Naturalis.

Explanation

Explanation of the Figures.

Central Projection of the Twelve Hour-lines, Figure 1st to Figure 3d.

Figure 1st, Is a central or gnomonic projection of the hectemorial lines or antique hour-lines, on the inside of a plane touching the sphere at the elevated pole of the equator. This and all the other figures are drawn for the latitude 66° 30′. Each antique hour-line is marked with the Greek numeral that belongs to it.

Figure 2d, Is a central projection of the summer portion of these lines on the inside of a plane touching the sphere at the depressed pole of the equator.

The circle placed between figure 1st and figure 2d is a great circle of that sphere from which all the projections in these figures (except figure 9th) are formed.

Figure 3d, Is a central projection of the hectemorial lines on a plane touching the sphere at the depressed intersection of the meridian and the equator.

Sections of the five undulated Cones which contain the antique hour-lines, by a plane at right angles to the axis, figure 4th to figure 8th.

Figure 4th, Is a section at right angles to the axis of the undulated conical surface, which contains the first antique hour-line A, and eleventh IA. In this and the other figures, the full lines are the section of one of the two opposite cones, and the dotted lines are the section of the other.

other. The section of the same undulated cone by a cylindrical surface, is seen in figure 10th.

Figure 5th, Is the right section of the undulated cone which contains the second antique hour-line B, and tenth I. The section, by a cylindrical surface, is seen at figure 11th.

Figure 6th, Is the right section of the undulated cone which contains the third antique hour-line Γ, and ninth Θ. Its section, by a cylindrical surface, is figure 12th. A perspective and shaded view of this undulated conical surface is figure 9.

Figure 7th, Is the right section of the undulated cone which contains the fourth antique hour-line Δ, and the eighth H; figure 13th is its section by a cylindrical surface.

Figure 8th, Is the right section of the undulated cone which contains the fifth antique hour-line E, and the seventh Z. Figure 14th, is its section by a cylindrical surface.

Perspective and shaded view of one of the Surfaces, figure 9th.

Figure 9th, Is a perspective and shaded view of the undulated conical surface which contains the third antique hour-line Γ, and ninth Θ; the point of sight is in the plane of the equator, and the distance of the eye is two diameters from the centre of the sphere; in this view only one of the two opposite surfaces is drawn. The right section of these opposite cones is figure 6th, and their section by a cylindrical surface is figure 12th.

Sections

Sections of the five undulated Conical Surfaces by a Cylindrical Surface, figures 10*th to* 14*th.*

Figure 10*th*, Is the section of the undulated cone which contains the first antique hour-line A, and eleventh IA, by a cylindrical surface touching the sphere at the equator. To avoid intricacy in this figure, the section of only one of the two opposite cones is drawn. Figure 4th is the right section of the two opposite surfaces.

Figure 11*th*, Is the section of the undulated cone which contains the second antique hour-line B, and tenth I, by a cylindrical surface. The full line is the section of one of the two opposite cones; the dotted line is the section of the other. Fig. 5. is the right section of this surface.

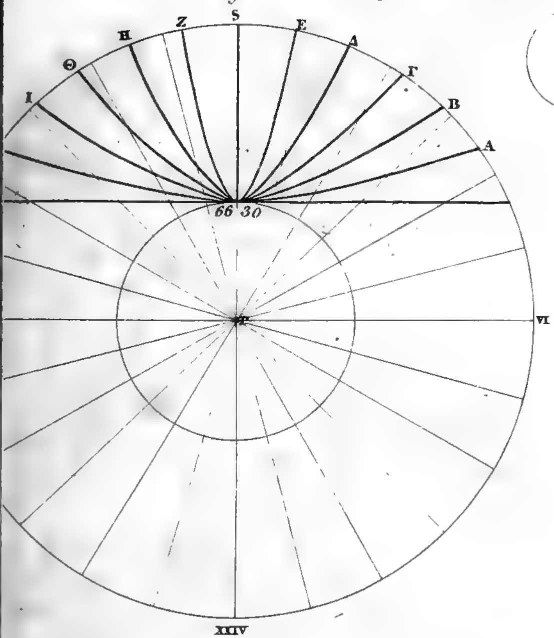
Figure 12*th*, Is the section of the two opposite surfaces which contain the third antique hour-line Γ, and ninth Θ, by a cylindrical surface. Their right section is figure 6th; and one of these two opposite surfaces is represented in perspective, and shaded at figure 9th.

Figure 13*th*, Is the section of the undulated cone which contains the fourth antique hour-line Δ, and eighth H, by a cylindrical surface. In this undulated cone, the two opposite conical surfaces coincide in one: there are therefore no dotted lines, neither on this figure, nor on figure 7th, which is the right section of this undulated cone.

Figure 14*th*, Is the section of the undulated cone which contains the fifth antique hour-line E, and seventh Z, by a cylindrical surface. Figure 8th, is the right section of the same.

PLATE II. *Engraved for the Royal Society Trans.s Vol 8. page 82.*

Fig. 1.



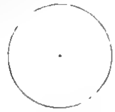


Fig. 2.

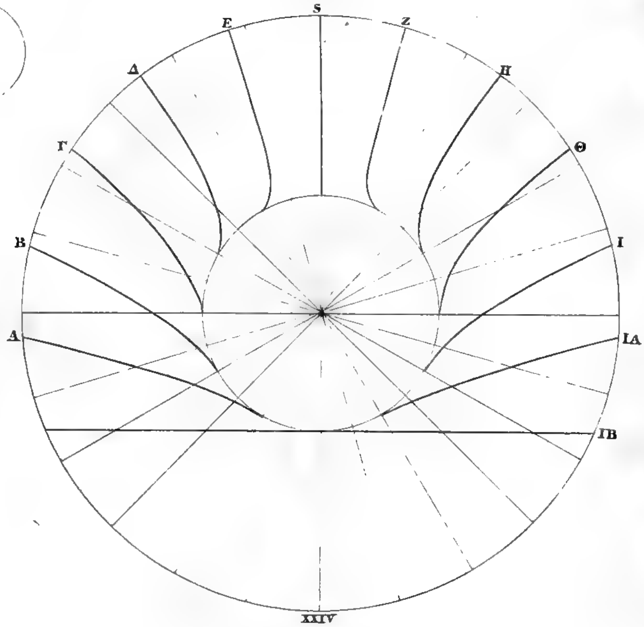


Fig. 3.

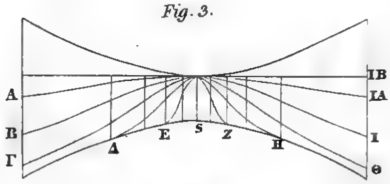


Fig. 4.

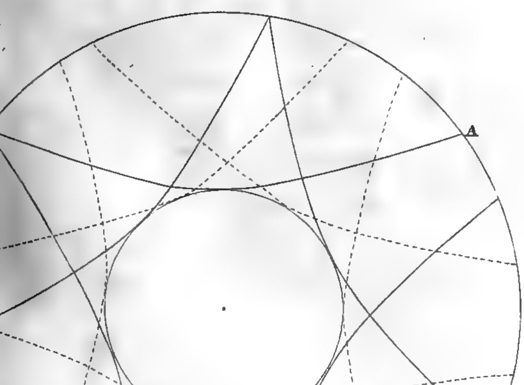
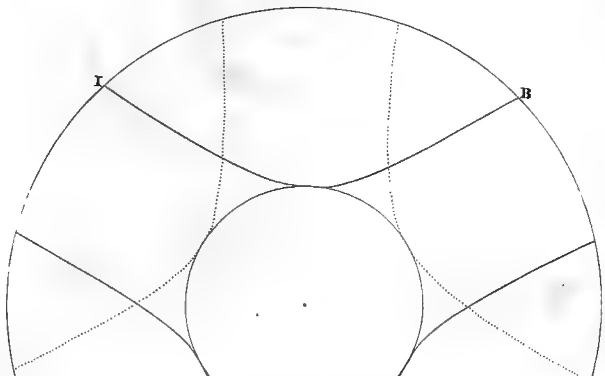


Fig. 5.



Engraved by W. & D. Lizars Edin.r

PLATE III.

Fig. 6.

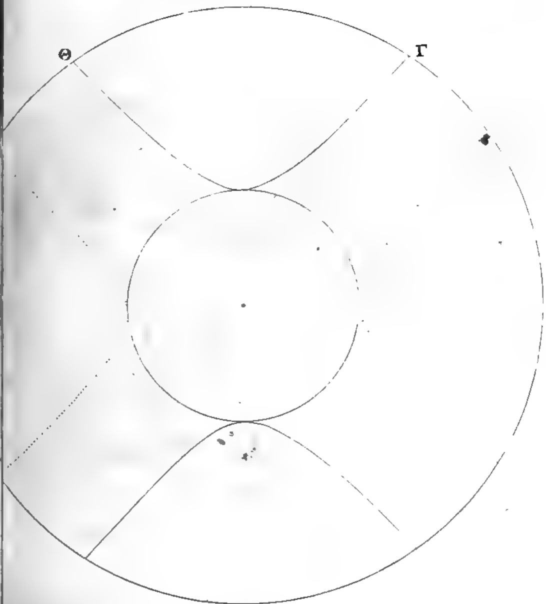


Fig. 7.

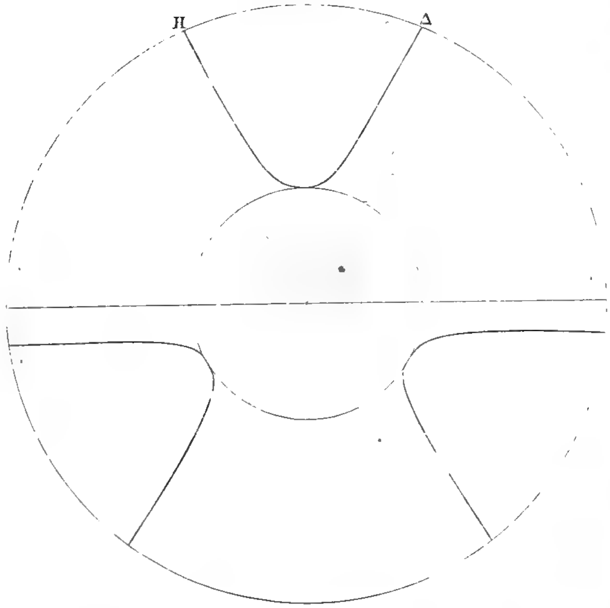


Fig. 8.

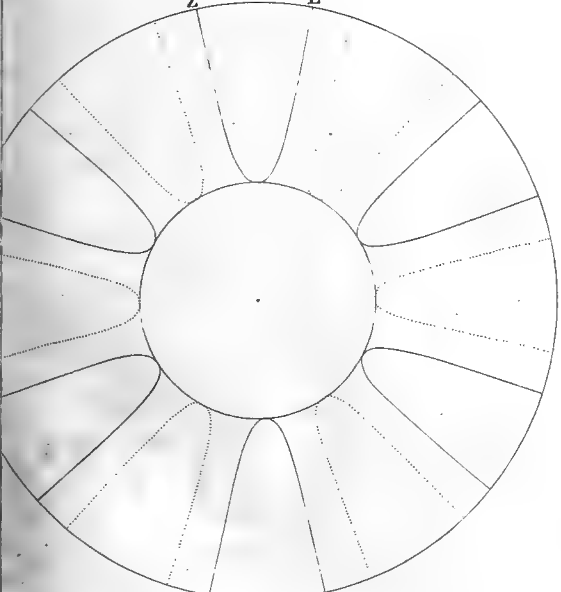


Fig. 9.

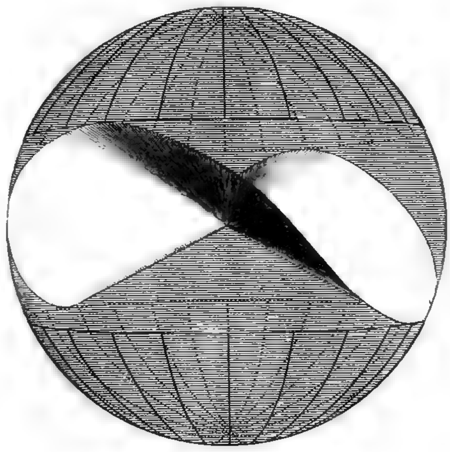


Fig. 10.

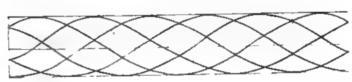
A, I A 

Fig. 11.

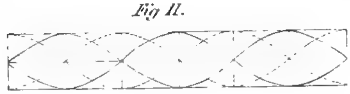
B, I 

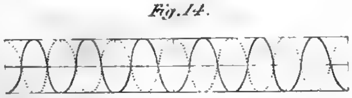
Fig. 12.

Γ, Θ 

Fig. 13.

Δ, Η 

Fig. 14.

E, Z 

V. *On the Origin of Cremation, or the Burning of the Dead.* *By* John Jamieson, D. D. F. R. S. & F. A. S. E.

(Read April 3. 1815.)

AS far as we can judge from historical records, the primeval mode of disposing of dead bodies, was by inhumation. It has been observed in another essay, that according to Pliny, the ancient Romans did not burn their dead, but consigned them to the earth *. It must be admitted, however, that by some the mode of cremation had been preferred in a very early period of their history; as we cannot otherwise account for the prohibition, which Plutarch ascribes to Numa, as to the burning of his body. If we may credit the testimony of Cicero, the Greeks, during the reign of Cecrops, inhumated their dead †. The same mode of interment is attributed, by Ælian, to the Athenians ‡; and by Plutarch to the Greeks in general ‖. It is well known that Cecrops and Danaus, who brought

colonies

* Plin. Hist. Nat. lib. viii. c. 54.

† De Leg. lib. ii.

‡ Var. Hist. lib. v. 14.; vii. 19.

‖ In Vit. Solon.

colonies into Athens and Argos, were Egyptians; and generally admitted that CADMUS, who founded Thebes in Bœotia, was a Phenician. It may therefore be conjectured, that as neither the Egyptians nor the Phenicians burnt their dead, while the memory of these illustrious men retained any considerable influence, the colonies planted by them would strictly adhere to the rites of sepulture which they had introduced; especially as these had been sanctioned by the authority and example of their ancestors, in the countries from which they had migrated. From the institutes of LYCURGUS it is evident, that, in his time, the ancient mode of inhumation prevailed among the Spartans. For he enacted, that the dead should be deposited in the earth, wrapt in a covering of scarlet cloth, and surrounded with olive leaves *.

It seems to be generally believed, that, in later ages, the Greeks universally burned their dead. LUCIAN, indeed, as POTTER has remarked in his *Archæologia*, expressly assigns cremation to Greece, and inhumation to the Persians †. But this must be understood with great latitude. From the language ascribed by PLATO to SOCRATES, it appears, that both these modes had been promiscuously used in his time, according to the predilection of individuals. For he speaks of it as a matter of indifference to him, whether, after his death, his body should be burned or buried. The language of ÆLIAN would imply, that inhumation had continued to be the general practice at Athens. For he says, "It is an Athenian law, that if "any one accidentally meet with the corpse of a man not bu-"ried, he shall cover it entirely with earth; and that the dead "shall

* PLUTARCH. in Vit. LYCURG.

† Διελόμενοι κατα ἔθνη τὰς ταφας, ὁ μὲν Ἕλλην, ἔκαυσεν. ὁδὲ Πέρσης, ἔθαψεν, &c. LUCIAN. de Luctu, Oper. ii. 306. edit. Amstel. 1687.

"shall be buried so that they may look towards the west *." Both these institutes obviously preclude the idea of cremation.

But although, in some of the Grecian states, the ancient custom seems to have long retained its influence, it cannot be denied, that, according to Homer, the mode of burning had been very common among the Greeks before the time of the Trojan war. From the particularity with which he describes the funeral rites of Patroclus, in the twenty-third book of the *Iliad*, there is no reason to suppose that he viewed them as even at that time a recent institution.

It would appear, indeed, that the Phrygians were acquainted with this custom before it was received by the Greeks †. But it can scarcely be supposed, that the Greeks borrowed it from them, either during the Trojan war, or in any preceding era. It is far more probable, that it had made its way from Thrace, where it unquestionably prevailed in an early age ‡; especially as it cannot reasonably be doubted, that a considerable, if not the greatest part of Greece, was peopled from that country. The Thracians, most probably, received this custom from their progenitors the Scythians, who inhabited those vast regions now known by the name of Tartary. As the people of that country, apparently in the most remote ages, erected very large *tumuli* in honour of the dead, it is undeniable, from the remains found in those which have been explored by the Russians or Tartars, that, in many instances at least, they burned their bodies. Mr Tooke, indeed, informs us, that no sepulchral urns have been discovered in any of these tombs.

"Of

* Ælian. Var. Hist. lib. v. c. 14.

† Alex. ab Alexandro, Gen. Dies, lib. iii. c. 2.

‡ Herodot. Hist. Terpsich. c. 8.

" Of these Russian and Siberian sepulchres, some (he says) " are encompassed with a square wall of large quarry stones, " placed in an erect position; others are covered only with a " small heap of stones, or they are tumuli adorned at top. In " many of these sepulchres the bones of men, and frequently " of horses, are found, and in a condition that renders it pro- " bable the bodies were not burnt before they were inhumed. " Other bones shew clearly that they have been previously " burnt; because a part of them is unconsumed, and because " they lie in a disordered manner, and some of them are want- " ing. Urns, in which other nations of antiquity have deposi- " ted the ashes of their dead, are never met with here. But " sometimes what remained of the bodies after the combu- " stion, and even whole carcases, are found wrapped up in thin " plates of gold.—There is a very remarkable circumstance " observable in some of the tombs on the upper part of the " Yenisei, which forms an exception to the general rule of " other sepulchres. Instead of ornaments and utensils of gold " and silver found in other tombs, you meet here only with " copper utensils. Even such instruments as would have been " better wrought of iron are here found all of copper, as " knives, darts and daggers. The nation, therefore, whose " dead are here inhumed, seems to have been unacquainted " with the use of iron; and these tombs must accordingly be " more ancient than the others *."

This learned writer seems himself to admit, that some of these monuments are far more ancient than others; and gives an indubitable proof of the high antiquity of some of them, when he remarks, that all the instruments found in them were made

* Account of the Burial-places of the ancient Tartars, by the Reverend WILLIAM TOOKE, F. R. S. Chaplain to the English Factory at St Petersburgh. *Archaeologia*, vol. vii. p. 223, 224.

made of copper; whence he reasonably concludes, that the use of iron had been unknown in those regions when these monuments were erected. This might seem to carry us as far back as to the times of the Massagetæ; who, as we learn from HERODOTUS, used no iron, having all their weapons made of brass *. From the same venerable historian, we learn the great respect which the Scythians had for the tombs of their ancestors. That intelligent traveller STRAHLENBERG informs us, that these monuments contain earthen urns of different sizes †. He does not say, however, that bones have been found in any of them. It affords a strong presumption that many tribes of the Scythians anciently burned their dead, that the Chinese Tartars, who are said to be the descendants of those Scythians whose tombs are to be seen on the river Jenisei, still retain this mode ‡.

Perhaps the first notices which we have of this custom, in ancient history, occur in the slender accounts that have been handed down to us concerning the manners of some of the nations of Hindostan. How early they burned their dead we are not informed. But we certainly know, that, before the time of ALEXANDER of Macedon, they erected funeral piles for the living. QUINTUS CURTIUS, from TROGUS, asserts, that those who were called Wise Men, when they saw the infirmities of age approaching, ordered their pyres to be raised, and cheerfully devoted themselves to the flames §. The same thing is asserted by CLEMENS of Alexandria concerning the Gymnosophists. Speaking

* Ὅσα μὲν γὰρ ἐς αἰχμὰς καὶ ἄρδις καὶ σαγάρεις, χαλκῷ τὰ πάντα χρέωνται. Clio, c. 215.

† Description of the North and East parts of Europe and Asia, p. 364, 365.

‡ Ibid. p. 365. 367.

§ Hist. lib. viii.

Speaking of those who unnecessarily exposed themselves to destruction, under the false idea of being martyrs, he adds, "In "vain do they give themselves up to death, as the Gymnoso- "phists of India rashly cast themselves into the flames *.

As it is acknowledged even by heathen writers, that the most ancient mode was that of inhumation, a question naturally occurs, which, although from deficiency of evidence it may be impossible to solve in a satisfactory manner, affords ample ground for curious and interesting disquisition. It is this; Whence might the practice of cremation originate? or, in other words, What could induce men, in opposition to the feelings of nature, to devote the mortal part of this frame, which they had cherished so tenderly during life, as far as possible to apparent destruction, after the departure of the spirit?

By the primitive Christians it was objected to cremation, that the practice involved in it the idea of inhumanity to the body. Hence Tertullian having remarked, that some of the gentiles disapproved of the mode of burning, because they wished to spare the soul, which hovered over the body after death, subjoins, "But we have another reason,—that of piety, "not as flattering the reliques of the soul, but as detesting "cruelty even to the body; because, being itself man, it does "not deserve to be subjected to a penal death †." In another place,

* Θανατῳ δὲ ἑαυτὲς ἀποδιδόασι κενῳ, καθάπερ καὶ ὁι τῶν Ινδῶν γυμνοσοφιςαὶ ματαιῳ πυρί. ἐπεὶ δὲ ὁι ψευδώνυμοι ὅτι τὸ σῶμα διαβαλλεσι, &c. Clem. Alexandr. Strom. lib. iv. p. 351. edit. Lugd. 1616.

† Et hoc enim in opinione quorundam est: propterea nec ignibus funerandum aiunt, parcentes superfluo animæ. Alia est autem ratio pietatis istius, non reliquiis animæ adulatrix, sed crudelitatis etiam corporis nomine aversatrix, quod et ipsum homo non utique mereatur pœnali exitu impendi. Tertullian. de Anim. c. 51.

place, he ridicules the heathen for their inconsistency, in first burning the dead in the most unfeeling manner, and then celebrating the feasts which they denominated *Parentalia*, by the same fires both honouring and insulting them, treating them as if they had been gluttons after they had consumed them. "O "piety!" he exclaims, "sporting itself in acts of cruelty *."

The adversaries of the Christians, indeed, objected to them, that they had a weightier reason for opposing cremation. MINUCIUS FELIX, accordingly, introduces the heathen as saying, "For this reason they execrate the funeral pile, and condemn "sepulture by burning," as if it precluded the possibility of resurrection. But MINUCIUS replies, "We do not, as you be-"lieve, fear any injury by this kind of sepulture; but we ad-"here to inhumation as the more ancient and the preferable "mode †."

From the ridiculous reason assigned by TERTULLIAN, for the reluctance which some of the heathen felt to cremation, it appears that they were actuated by the self-same feeling with Christians; although, according to this ancient writer, they transferred their compassion from the body to the soul. But some of them, it is evident, viewed the practice as inhuman on

* Ego magis ridebo vulgus, tunc quoque quum ipsos defunctos atrocissime exurit, quos postmodum gulosissime nutrit, iisdem ignibus et promerens et offendens. O pietatem de crudelitate ludentem! Id. de Resurrect. c. i.

† Inde videlicet et execrantur rogos, et damnant ignium sepulturas, quasi non omne corpus, etsi flammis subtrahatur, annis tamen et ætatibus in terram resolvatur.—Hoc errore decepti beatam sibi, ut bonis, et perpetem vitam mortuis pollicentur; cæteris, ut injustis, pœnam sempiternam. MIN. FEL. Octavius, p. 97, 98. edit. Lugd. 1672. Nec, ut creditis, ullum damnum sepulturæ timemus, sed veterem, et meliorem consuetudinem humandi frequentamus. Ibid. p. 327, 328.

a more general ground. Hence a poet, cited by Eustathius, introduces a person as exclaiming against it, and as invoking Prometheus to hasten to his assistance, and snatch, if possible, from mortals, the fire with which he had supplied them *.

As Jews and Christians, in every age, preferred inhumation, because it bore a more direct analogy to the origin of man, it is remarkable, that even Lucretius virtually assigns the same reason for the practice:

> Cedit enim retro, de terra quod fuit ante
> In terram.——

A variety of reasons may be supposed, either separately or in connection with each other, according to the prejudices and habits of particular tribes or families, to have induced the introduction of this mode. The influence of such reasons may also be supposed to have been greater or less, in proportion to their relative probability, as they appeared to the minds of those who contemplated them; or according to peculiar circumstances, as existing in different ages, or among different nations. There are other reasons, which have a superior claim to our attention, as being expressly mentioned by ancient writers.

1. The mode of cremation may have been preferred, in some instances, as a means of guarding the living against the fatal effects of putridity from the dead. The Romans, we know, originally used their dwelling-houses as tombs for their deceased relations. The same practice prevailed among the more early Greeks †. The Thebans, in one period of their history, had

* Eustath. in Iliad. A. p. 32.

† Οἱ δὲ αὖ ἐκείνων πρότεροι αὐτοῦ καὶ ἔθαπτον ἐν τῇ οἰκίᾳ τοὺς ἀποθανοντας; Ἡμεῖς δὲ τούτων οὐδὲν ποιοῦμεν. Plat. Minoc, Oper. ii. p. 315. edit. Paris. 1578.

had a law, that no one should build a house without providing a repository for his dead*. The disastrous consequences of this unnatural approximation, as they were strangers to the art of embalming, might be felt for a considerable time, before individuals could find it possible to release themselves from the fetters of custom, strengthened by superstition. But as men advanced in civilization, this strange practice would become matter of cognisance to those whose office obliged them to watch over the public weal; and we may naturally enough suppose, that the prohibition of domestic sepulture would be an intermediate step between the observation of this custom, and the enactment of that law which forbade the Romans either to bury, or to burn, the bodies of the dead within the city. It may be observed by the way, indeed, that they, with many other ancient nations, as well as the Chinese, have manifested much more common sense and delicacy in this respect than the nations of modern Europe, notwithstanding their boasted refinement.

2. Those who wished to preserve the remains of the dead as long as possible, in token of regard for their memory, might prefer this mode to inhumation. Knowing that, in consequence of interment in the common way, the bones themselves gradually moulder into dust, till every vestige of the person be lost, they might in some instances adopt the plan of calcination, as a means of partially preserving them. There can be no doubt that this was the most eligible mode, where embalming was not used, when it was meant to transport the remains of the dead from one place to another. As the Greeks ascribe the introduction of cremation to HERCULES, they in effect assign, as the reason of his burning the body of ARGIUS, that he was under

* V. POTTER's Archæol. ii. p. 218. Lond. edit. 1751.

der the necessity of transmitting it to his father LICYMNIUS, as the only way in which he could fulfil the promise he had made to restore his son.

3. Before cremation was generally adopted by any nation, it might be found necessary during the prevalence of any pestilential disease. In the passage in which HOMER first mentions this mode as observed by the Greeks, although he does not directly assign the pestilence as the reason of its being used, he ascribes the frequency of the kindling of the funeral pile to the prevalence of that contagion which was viewed as the effect of the wrath of APOLLO:

Αὐτὰρ ἔπειτ' αὐτοῖσι βέλος ἐχεπευκὲς ἐφιεὶς,
Βαλλ' αἰεὶ δὲ πυραὶ νεκύων καίοντο θαμειαί.

Iliad. A. 51.

From a comparison of this passage, indeed, with what the poet has said in his introduction, it may be inferred, that cremation was supposed to be more peculiarly necessary in this case; as he there speaks of the multitude of heroes that fell in battle, who were left a prey to dogs and to all the fowls of heaven:

——— *'Αυτὲς δὲ ἑλώρια τεῦχε κύνεσσιν,*
'Οιωνοῖσί τε πᾶσι.

It appears that the Hebrews, though they adhered to the more ancient mode, had no objection to burn the dead in a time of contagion. In this sense are we to understand the language of the prophet AMOS, when foretelling the consequences of a continued famine: "A man's uncle," or near relation, "shall take him up, and he that burneth him, to bring "out

"out the bones out of the house, and shall say unto him that "is by the sides of the house, Is there yet any with thee *?" Though they carried the bones to the sepulchres, they did not venture to bring out the dead body before cremation, lest others should be infected.

From several passages in the Sacred Writings, it has been inferred, that the Jews in a later age actually burnt their dead. But it is to be observed, that as the language refers exclusively to their kings, it is not said that they burnt their bodies, but only that they "made burnings *for* them †." Even from the account which is given of the honours paid to the memory of Asa, though it has been urged as a proof that the Jews had adopted cremation, it is evident that the burning was a ceremony superadded to his interment; for it would appear that his body was wrapped in spices: "They buried him in his "own sepulchre, which he had made for himself in the city of "David, and laid him in the bed," the coffin or sarcophagus, as would seem, "which was filled with sweet odours and di-"vers kinds of spices prepared by the apothecaries art; and "they made a very great burning for him ‡." The writers of the *Ancient Universal History* suppose, that these spices were burnt around the body §. But this does not seem likely; for we cannot well conceive, how, in this case, the body itself should not have been affected by the force of the fire. It may, therefore, more naturally be supposed, that this burning did not take place in the sepulchre. Perhaps, though the Jews did not consume the body, yet on such occasions they so far complied

* Amos, vi. 10.

† 2 Chron. xxi. 19.; Jer. xxxiv. 5.

‡ 2 Chron. xvi. 14.

§ Vol. iii. p. 173.

plied with the customs of other nations, as to erect a funeral pile at the entrance of the tomb, which they might burn in honour of the dead, containing nothing save spices and odoriferous woods, or having an image of the deceased prince substituted for the body. It cannot reasonably be thought, that they would have accounted that an honour to their kings, which was deemed a disgrace to every other person. Not only in the more early period of their history *, but even towards the dissolution of the monarchy, the burning of the dead body was viewed as a sort of posthumous punishment, expressive of the greatest contempt and detestation. For JOSIAH, we are informed, " burned the bones of the priests of BAAL upon their " altars †."

4. It tended greatly to facilitate the reception of this custom, that it seemed the most certain plan for protecting the dead body from those indignities to which it might otherwise have been subjected. It is highly probable, indeed, that the danger to which the bodies of departed relations was exposed, of being disfigured or devoured by beasts of prey, first suggested the idea of covering their graves with heaps of stones; and that this course had been followed in a very early stage of society. But when war had begun to extend its cruel ravages, when man had become as unfeeling to his fellows as the tyger or the hyæna, when his ferocity reached even beyond the hallowed precincts of the tomb; it would be found necessary to devise some more effectual plan for securing rest to the dead.

It has been observed, accordingly, in another dissertation, that, as PLINY informs us, the Romans adopted cremation in consequence of being engaged in distant wars; and also, that SYLLA,

* JOSH. vii. 25.

† 2 Chron. xxxiv. 5.

SYLLA, contrary to the custom which had hitherto been religiously observed in the Cornelian family, ordered his body to be committed to the flames, lest the dishonour done by him to MARIUS might be retaliated. For a similar reason, undoubtedly, the valiant inhabitants of Jabesh-gilead "arose, and went "all night, and took the body of SAUL, and the bodies of his "sons, from the wall of Beth-shan, and came to Jabesh and "burnt them there *." They deeply felt the dishonour that had been already done to their deceased sovereign and his gallant sons, whose bodies had been "fastened to the wall of "Beth-shan," one of the cities of the Philistines, that they might be exposed to every species of outrage from their remorseless adversaries. They, therefore, carried them off and burned them; not with the intention of giving them the funeral honours of cremation, but merely to prevent the possibility of their being hung up as before. This, they had every reason to suspect, would have been the case, had they interred them in the usual way, because the land of Palestine was at this time completely under the power of their enemies, and circumstances did not permit them to carry the bodies across Jordan. They gave them, as far as possible, the common rites of sepulture, by interring their remains; but not till they had done what seemed previously necessary for guarding against a repetition of similar indignities.

5. Some of the ancients preferred this mode, because, according to their ideas (whether well or ill founded, it is not our business here to inquire), it most speedily reduced the body to its first principles. THALES, and his followers, viewed water as the origin of all things; and therefore reckoned it most fit that the body should, by putrefaction, be reduced to its original element. HIPPON, as we learn from TERTULLIAN, taught the same doctrine.

* 1 SAM. xxxi. 10,—13.

doctrine *. HERACLITUS, who is said to have flourished in the time of DARIUS HYSTASPES, held fire to be the first principle. Hence he, with those of his sect, preferred cremation †. MACROBIUS mentions two of this name, whose doctrine was materially the same. HERACLITUS of Pontus, he says, affirmed that the soul was light; and HERACLITUS the natural philosopher, that it was the scintillation of a stellar essence ‡. "The old "heroes in HOMER," says Sir THOMAS BROWN, "dreaded no-"thing more than water or drowning; probably upon the old "opinion of the fiery substance of the soul, onely extinguish-"able by that element; and therefore the poet emphatically "implieth the total destruction in this kinde of death, which "happened to AJAX OILEUS ||." Hence SERVIUS, in reference to the horror expressed by ÆNEAS in prospect of being shipwrecked, remarks, that this was not from the fear of death, but from the apprehension that his soul might perish, if his body should have no other than a watery grave §.

Several ancient writers held fire in such estimation, that they considered it as animated, and therefore as not to be extinguished

* TERTULLIAN. de Anima, c. 4.

† *Vide* ALEX. ab ALEXANDRO Genial. Dies, lib. iii. c. 2. POTTER's Archæol. ii. p. 207.

‡ HERACLITUS Ponticus lucem; HERACLITUS physicus, scintillam stellaris essentiæ. In Somn. Scipion. lib. i.

|| Hydrotaphia, p. 4.

§ Commenting on these words in the first book of the Æneid,

> Extemplo ÆNEÆ solvuntur frigore membra,
> Ingemit, &c.——he says,

Non propter mortem, sequitur enim, *O terque quaterque beati,* sed propter mortis genus. Grave est enim secundum Homerum, perire naufragio, quia anima ignea est: et extingui videtur in mari, id est, elemento contrario.

tinguished *. Bartholine has observed, that the ancient Scandinavians were influenced by a similar idea, how much soever it is enveloped in the darkness of their mythology. As they believed the principal heaven to be fiery, they adored that sacred fire which it was impious to extinguish. Fire being also considered as eternal, they assured themselves that the soul, which was loosed from the body by means of this honoured element, would most certainly, and in the most expeditious mode, be conveyed to the seats of the blessed. As Odin had enjoined cremation on his followers, they were persuaded that the honour, with which the person whose body was burned would be received into heaven, would be exactly in the ratio of the height to which the flame of his funeral pile ascended. As they denominated the rainbow "the bridge of "the gods," by which it was necessary that men should ascend into the celestial regions, they affirmed that the red division consisted of fire †.

This learned writer gives it as his opinion, that Odin, whom the northern nations worshipped, was the Sun; that the souls of heroes were said to be received by him, because they believed that the soul, being of an igneous essence, was a particle derived from their deity; and that the warrior, who led the *Asæ*, or Asiatic chiefs into Scandinavia, assumed this name, by which the sun was known in eastern regions, that he might ensure divine honours to himself. He thinks that it was with this design that he ordered his body to be burned, that his subjects, following his example, might be supposed, after death, to be translated by fire to a state of eternal fellowship with the object of their adoration.

* V. Scacchii Myrothecium, lib. i. c. 9. p. 46.

† V. Bartholin. de Causis Contempt. Mort. p. 272,—274.

6. The reception of this mode has been also ascribed to the influence of the ancient tradition, that the world should be destroyed by means of fire. The very ingenious and learned physician quoted above, observes, in that light and rather affected sort of style which characterizes his writings; "Such as "by tradition or rational conjecture held any hint of the final "pyre of all things, or that this element must at least be too "hard for all the rest, might conceive most naturally of the "fiery dissolution *."

That this tradition was not only known to some of the Greek philosophers, but even received by some of them, is unquestionable. PLATO, when giving an account of the conference which SOLON had with the Egyptian priests at Sais, acknowledges that they spoke in the most contemptuous terms of the learning of the Greeks, asserting that they were always children, that there was no old man among them, as they still possessed juvenile minds. "For, (said one of the senior priests,) "you have no ancient doctrine from old tradition, nor any dis-"cipline that has become hoary with age:" adding, "The "greatest dissolutions of the world have originated, and will "originate, from fire and water †." SOLON having proposed some queries in regard to DEUCALION, PYRRHA, PHAETON, &c. the priest proceeded to describe the dissolution by fire, in reference to the mythological account of PHAETON having set the world on fire. "This (he said) has indeed the air of a fable; "but the truth is, there shall be a great parallax," or "change, "in heaven and in earth; and in a short time the dissolution "of terrestrial things shall be effected by much fire ‡."

This

* BROWN's Hydriotaphia, p. 3.

† Πολλαὶ δὲ κατὰ πολλὰ φθοραὶ γεγόνασιν ἀνθρώπων, καί ἔσονται, πυρὶ μὲν δὲ ὕδατι μέγισαι. TIMÆUS, Oper. iii. p. 22.

‡ Τῦτο μύθου μὲν σχῆμα ἔχον λέγεται, τὸ δ' ἀληθές ἐστι, τῶν περι γῆν καὶ κατ' οὐρανὸν ἰοντων παράλλαξις, καὶ διὰ μακρῶν χρόνων γινομένη τῶν ἐπι γῆς πυρὶ πολλῷ φθορά. Ibid.

This, it has been observed, was the general doctrine of the Platonists. It was also held by other philosophers, especially by the Stoics. It has been asserted by the learned Grotius, that Zeno of Cittium, the founder of the sect of the Stoics, received this opinion from the Phenicians; as Cittium was a Phenician colony in Cyprus. Whether this remark be well founded or not, the doctrine was generally adopted by his followers. Seneca expressly asserts, that, as "the world had its origin by "water, it shall be destroyed by fire *." He says in another place; "All things shall fall by their own power; the stars shall "rush on the stars, and universal matter shall blaze in one "fire. Whatever now shines in the world shall then be in "flames †."

Diogenes Laertius thus expresses the doctrine of Heraclitus; "There is one world, which was produced by fire, and "shall be again reduced into fire ‡." Need I add the well-known language of Ovid?

> Esse quoque in fatis reminiscitur, affore tempus
> Quo mare, quo tellus, correptaque regia cœli
> Ardeat; et mundi moles operosa laboret.
>
> *Metam.* lib. i. ver. 256.

It ought to be observed, however, that neither this, nor the preceding reason, assigned for the introduction of cremation, can be viewed as satisfactory. For it appears that this custom

was

* Ita ignis exitus mundi est, humor primordium. Natural. Quæst. lib. iii. c. 13.

† Epist. de Consolat. ad Polyb.

‡ Ἕνα εἶναι κόσμον, γεννᾶσθαι τε ἀυτον ἐκ πυρὸς, καὶ πάλιν ἐκπυροῦσθαι. V. Gale's Court of the Gentiles, b. iii, c. 7.

was pretty general in Greece before the existence either of THALES or of HERACLITUS, and, indeed, before either of their peculiar systems had been broached. It seems highly probable, indeed, from what HOMER has said in regard to the fate of AJAX, that those philosophers who honoured fire as the primary element, had built their hypothesis on ancient tradition.

Besides, it is attested by universal experience, that the theories of philosophers have had very little influence on the manners and customs of mankind. In many instances they have endeavoured, in their own way, to account for certain customs which prevailed among their countrymen, or in other nations; and have occasionally accommodated their systems to those modes which had the sanction of antiquity. But it may well be questioned, if, in any one instance, the dogma of the most celebrated school has given rise to a rite or custom which has been generally received by the multitude. The influence of their authority was almost entirely confined to their own disciples; and, while a theory directly opposite was keenly supported by others, who claimed equal authority, and who had an equal right to exhibit this claim, the surrounding multitude could only stare at the supposed wisdom of contending sects, without attempting to decide the controversy, or even supposing that they were qualified for so arduous a task.

We must inquire, therefore, if there was no idea pretty generally received among men, that entwined itself with their religious creed, and derived peculiar influence from their hopes and fears, as to a state of future existence; which, if it did not absolutely originate this custom, must have greatly facilitated its progress. We accordingly observe,

7. That the body was believed to be unclean after the departure of the soul; and that it was therefore deemed necessary that it should be purified by fire. This is given by EUSTATHIUS

as one reason for the general reception of this custom in Greece. " There was," he says, " a certain purification by the " consumption of fire, because fire is a purificator; wherefore " purifications were made by fire." And Euripides gives a similar statement, when he says, " that the body of Clytemnes- " tra was purified," literally, " sanctified by fire *." The idea of pollution by the dead seems to have been early diffused: and this idea presupposes that of the body, as separated from the soul, being itself unclean.

" Not the Jews only," says the accurate and learned Potter, " but the greatest part of the heathen world, thought " themselves polluted by the contact of a dead body; death " being contrary to nature, and therefore abhorr'd by every " thing endued with life †." Among the Greeks, as long as there was a corpse in any house, a vessel of water was placed before the door, that those who had had any communication with the dead body, might, before their departure, purify themselves by washing. Hence Euripides makes the chorus call in question the death of Alcestes, because this customary signal was not exhibited.

> Πύλων πάροιθεν δ' ουχ' ὁρῶ
> Πηγᾶιον, ὡς νομίζεταί
> Γε, χέρνιβ' ἐπὶ πρόθυρα πύλαις·
>
> *Alcestid.* vers. 69.

They supposed that even the house in which the corpse lay was not free from pollution. The same poet therefore introduces

* Αγνισμὸς δὲ τις ἦν ἡ διὰ πυρος δαπάνη τοῦ νεκρῶθεντος. ὀτι καὶ τὸ πῦρ ἁγνιςικον· διο καί οι καθαρμὸι διὰ πυρος ἐγινοντο. καὶ Ευριπίδης δὲ τοιουτον τι ἐκφανοι, ὅτι ου φησιν ὁτι τὸ της Κλυταιμνηςρας δεμάς, πυρι καθηγνιςαι. Eustath. in Iliad. A. ver. 52.

† Archæolog. ii. p. 188.

duces HELEN as saying, "Our houses are pure, not being defi-"led by the death of MENELAUS *." To this idea, of defilement being contracted by approximation to the dead, we ought undoubtedly to trace the custom which prevailed among the Romans, of thrice sprinkling with consecrated water all the relations of the deceased, and all who had attended the funeral; who were then said to have received expiation or lustration. This ceremony was performed by a priest, after the burnt bones had been put into an urn, and immediately before he gave the company leave to depart, by pronouncing the words of valediction.

> Ossaque lecta cado texit Chorinæus aheno:
> Idem ter socios purâ circumtulit undâ,
> Spargens rore levi, et ramo felicis olivæ,
> Lustravitque viros, dixitque novissima verba.
>
> VIRG. *Æn.* lib. vi.

As a further purification, those who had attended the obsequies of the dead, after being sprinkled with water, as we learn from FESTUS, stepped over a fire. This act was called *Suffitus*. The house was also purified, and swept with a particular kind of besom. The *Feriæ Denicales* were certain ceremonies instituted with this design. The *Flamen* of JUPITER was laid under the same restrictions as the Jewish High-priest. He was not permitted to touch the dead, or even to approach a grave †. The ancient Scythians themselves, though strangers to any species of refinement, supposed that they necessarily contracted defilement from the dead. HERODOTUS relates accordingly,

* EURIPID. *Helen.* vers. 1446.

† AUL. GELL. Noct. Attic. lib. x. c. 15.

ly, that they purified themselves, first by washing their heads, and then by burning hemp-seed on red-hot stones, with which they produced a powerful fumigation *.

It is well known that the Romans viewed fire and water as the two great principles of purification, and that with this design they applied them to things of every description. Hence Ovid, when describing the *Palilia*;

> Certe ego transilui positas ter in ordine flammas;
> Virgaque roratas laurea dedit aquas.
> *Fast.* lib. iv. v. 727.

They extended the purifying virtue of fire not only to their flocks, but to the owners of them.

> ——Per flammas saluisse pecus, saluisse colonos.
> Ibid. v. 805.

He proceeds to enumerate the different theories which had been formed, in order to account for the use of fire and water with this view. The variety of these, he says, caused hesitation as to the real origin. By some it was supposed, that as fire purified every thing, particularly metallic substances, it would have the same effect on the shepherd and his flock.

> Omnia purgat edax ignis, vitiumque metallis
> Excoquit. Idcirco cum duce purgat oves.
> Ibid. v. 785.

Others imagined, that as all things had their origin from fire and water, their ancestors had conjoined these elements in their rites of purification; while there were some who traced this practice to a conviction that in these existed the principle of life.

* *Melpom.* c. 73. 75.

life. Others again, he says, understood the one rite as referring to PHAETON, and the other to the flood of DEUCALION.

——Sunt quia Phaëthonta referri
Credant, et nimias Deucalionis aquas.
Ibid. v. 793.

PIERIUS, in his *Hieroglyphica*, quotes the language of PLAUTUS, as affording a proof that the ancient Romans ascribed a purifying virtue to fire.

Quid impurate, quanquam Volcano studes,
Cœnæ ne causa, aut mercedis gratiâ,
Nos nostras ædes postulas comburere?
Aulular. ap Pier. fol. 343. E.

PIERIUS, however, remarks, that they made a distinction between the use of fire and water, viewing the one as the means of purification, the other of expiation. *Ignis autem, ut nostri veteres tradidere, purgat; aqua expiat, lustratque.*

As both these modes of purification were practised long before the Romans had a national existence, it is not surprising that the ritual poet found himself quite at a loss to account for their origin. Though I do not pretend to determine from whom the Romans received these rites; yet the analogy, not only in the use, but in the conjunction of these, with that ordinance given to the Hebrews, with respect to the spoils taken in war, is too striking to be passed over in silence: "The gold "and the silver, the brass, the iron, the tin and the lead, every thing that may abide the fire, ye shall make it go through "the fire, and it shall be clean; nevertheless, it shall be purified with the water of separation: and all that abideth not "the

"the fire, ye shall make go through the water *." In the removal of defilement by the dead, the use both of fire and of water was necessary. A red heifer was to be killed, and completely burnt without the camp; the ashes of which were to be carefully collected, and laid up for future use in regard to all who had "touched a dead body, or a bone of a man, or a "grave." The law, enjoining the necessary purification, is thus expressed: "For an unclean person," that is, one polluted by the dead, "they shall take of the ashes of the *burnt* heifer of purification," or of atonement for sin, "and *running water* shall be put thereto in a vessel: and a clean person," a priest, according to the *Targum* of JONATHAN, "shall take hyssop, and dip it in the water, and sprinkle it upon the tent, "and upon all the vessels, and upon the persons that were "there, and upon him that touched a bone, or one slain, or "one dead, or a grave: and the clean person shall sprinkle "upon the unclean on the third day, and on the seventh day: "and on the seventh day he shall purify himself, and wash his "clothes, and bathe himself in water, and shall be clean at "even †." PIERIUS, as we have seen, while he marks the designed distinction between the use of fire and that of water, says that the fire was viewed as purifying, the water as possessing the power of expiation. This, however, was reversed among the Hebrews. The ashes of the burnt heifer seem to have respected the expiation of guilt, and the running water the removal of pollution. Not only in the Hebrew, but in the Roman ritual, sprinkling was expressly enjoined. According to both, it was administered by similar means. In the one case, a branch of olive, the emblem of peace and reconciliation, was used instead of a bunch of hyssop in the other.

* Num. xxxi. 22, 23.

† Num. xix. 1,—19.

It is well known, that, among some of the ancient nations of the East, parents were wont to consecrate their children to Moloch, whom some suppose to be the Sun, and others Saturn, by actually giving them up to the devouring flame. The infatuated parents persuaded themselves, that by this inhuman act they would secure their own prosperity, and that of the rest of their offspring. But this was not the only mode of consecration. We learn from MAIMONIDES, that, a great fire being kindled, the parent delivered his son to the priest, who had the charge of this fire; and that he gave him back to the father, in token of his being permitted to make him pass through the fire. After this ceremony, the father himself led his child through the flames, from one side of the fire to the other *. Correspondent with this account, the language containing the prohibition of this crime, in Deut. xviii. 10. is rendered in the Septuagint, " There shall not be found among you any one " that purifieth his son or his daughter in fire †."

The Druids, we are told, on May-eve kindled two great fires, between which the men and beasts, which were to be sacrificed, were made to pass in order to their purification ‡. In Ireland to this day, at the Feast of Beltein, which is held at the summer solstice, as they kindle fires on the tops of hills, every member of a family is made to pass through the fire; this ceremony being deemed necessary to ensure good fortune through the

* MAIMON. de Idololat. c. 6. s. 3.

† Περικαθαίρων τὸν ὑιὸν ἀυτȣ καὶ τὴν θυγατέρα ἀυτȣ ἐν πυρὶ.

‡ " Two fires were kindled by (near) one another, on May-eve, in every village of the nation, (as well thro'out all Gaule, as in Britain, Ireland, and the adjoining lesser islands,) between which fires the men and the beasts to be sacrific'd were to pass." TOLAND's Hist. of the Druids, Lett. ii. § 4.

the succeeding year *. As the designation of this feast retains the name of *Baal* or *Bel*, I need scarcely remark the striking resemblance of this rite to the ancient worship of the false god who was thus denominated by the nations of the East. There is great reason, indeed, to believe, that Baal and Moloch were merely different names for the same idol; the one word signifying *Lord*, and the other *King*. For those, who in one place are said to have " built the high places of Baal,—to cause their " sons and their daughters to pass through the fire unto Mo-" loch †," are said, in another, to have " built the high " places of Baal, to burn their sons with fire for burnt offer-" ings unto *Baal* ‡." There can be no doubt that the worship of Baal was that of the sun, who was designed " the lord," and " the king, of heaven."

The ancient Goths ascribed a similar virtue to this element. When, in their religious feasts, they drunk in honour of their gods or departed heroes, they stood around a great fire in the midst of the temple, and caused the cups, filled with wine or mead, to be passed through the flames ||.

PENNANT takes notice of a singular superstition, which still remains in the Highlands of Scotland, and which must certainly be viewed as a remnant of druidical worship. " It has hap-" pened," he says, " that, after baptism, the father has placed

* " Thus I have seen the people running and leaping through the St John's fires in Ireland; and not only proud of passing unsing'd, but, as if it were some kind of lustration, thinking themselves in a special manner blest by this ceremony, of whose original, nevertheless, they were wholly ignorant, in their imperfect imitation of it." TOLAND, ibid. § 7. V. also BORLASE's Antiquities of Cornwall, p. 130.

† JER. xxxii. 35.

‡ JER. xix. 5.

|| STURLESON, Vit. Haquin. Adalstan. V. KEYSLER. Antiq. Septentr. p. 355, 356.

" a basket, filled with bread and cheese, on the pot-hook that
" impended over the fire in the middle of the room, which the
" company sit around, and the child is thrice handed across
" the fire, with the design to frustrate all attempts of evil spi-
" rits, or evil eyes. This," he adds, " originally seems to have
" been designed as a purification, and of idolatrous origin, as
" the Israelites made their children to pass through the fire to
" Moloch *."

The mode of performing this unhallowed rite is different in some parts of the Highlands. One holds the new-born child by the shoulders, and another by the feet, while they shove it backwards and forwards across the fire. This is sometimes used as a test, whether the child be of the right blood, or merely a fairy urchin substituted in lieu of the genuine offspring. If, after this operation, the child, on being put to bed, fall into a copious perspiration, it is viewed as an infallible proof that there has been no elvish imposition. It must be admitted, indeed, that scarcely any method could be adopted more likely to ensure the wished for favourable omen. It may be observed, that, in the investigation of ancient superstitions, we have many examples of a change of the reason assigned for a peculiar rite, especially if there has been a change of the religious creed of a people; when there is no ground to doubt that the rite itself has remained unaltered.

I have met with one superstition in the low country, (for it still exists in the county of Angus), which seems to claim the same origin. A burning coal is put into the water in which a new-born child is to be washed. Were this important ceremony neglected, it is believed by many that the infant could not *thrive.*

The

* Tour in Scotland, 1772, p. 46.

The language of VIRGIL proves, that the Romans were not strangers to the same kind of idolatry. As the mountain Soracte was consecrated to APOLLO, his votaries manifested their ardour in his service, by rushing through the burning coals of the fire lighted up in honour of their god.

> Summe deum, sancti custos Soractis Apollo,
> Quem primi colimus, cui pineus ardor acervo
> Pascitur, et medium freti pietate per ignem
> Cultores multa premimus vestigia pruna;
> Da pater hoc nostris aboleri dedecus armis
> Omnipotens.—— *Æn.* lib. xi.

SERVIUS explains the term *acervo*, "Pyra coacervatione lig- "norum." Perhaps he did not use the word as denoting a funeral-pile, as it also signifies a bonefire. There is indeed no reason for supposing, that VIRGIL referred to the burning of the dead. We learn the meaning of this language from the testimony of PLINY: "Haud procul urbe Roma in Faliscorum "agro familiæ sunt paucæ, quæ vocantur HIRPIÆ: quæ sacrifi- "cio annuo, quod fit ad montem Soractem APOLLINI, super "ambustam ligni struem ambulantes non aduruntur." *Nat. Hist.* lib. vii. c. 2. The same species of worship is described by SILIUS ITALICUS, lib. v. ver. 175.

I need scarcely observe, that, as APOLLO was the same with the Sun, the passage affords a proof of the striking analogy between his worship among the Romans, and that which has been already illustrated. Did we know the particular day annually consecrated in this manner, the coincidence might be still more remarkable.

The early Greeks ascribed the same efficacy to fire. HOMER, accordingly, makes ULYSSES, after the slaughter of those who sought

sought to rival him in the affections of PENELOPE, to purify his house from blood by the use of fire and sulphur:

> Οἶσε θέειον γρηῦ κακῶν ἄκος, οἶσε δὲ μοι πῦρ
> Ὄφρα θεειώσω μέγαρον.
> *Odyss.* lib. xxii. v. 481.

As we certainly know, that the ordeal by fire was used in the times of heathenism, there is no reason to doubt that it originated from the same persuasion as to the purifying virtue of fire. The person who passed without injury, bare-footed and blind-fold, over nine glowing ploughshares, was said to be *purged* from the crime of which he had been accused. The names given, in various instances, to this species of trial, expressed the same idea. In the language of Iceland it was denominated *skirsla*, from *skir-a* purgare. The learned LUND traces the Gothic term *ordel*, *urdel*, perhaps rather fancifully, to Heb. *ur* ignis, as denoting judgment by fire *. In the Latin of the dark ages, the act was designed *purgatio vulgaris*; and the person was said, *per calidum ferrum se purgare*, or *ferro candenti se purificare*. It may be added, that, as the other mode of ordeal was by water, whether cold or hot, it appears that the principal tests of imputed criminality bore a strict analogy to the two great means of purification acknowledged by the ancients,—fire and water.

As a proof that the ordeal by fire was a remnant of pagan worship, the justly celebrated DU CANGE refers to the testimony of SOPHOCLES.

> Ἦμεν δ' ἕτοιμοι καὶ μύδρους ἄρειν χεροῖν,
> Καὶ πῦρ διέρπειν, καὶ θεοὺς ὁρκωμοτεῖν.
> *Antigon.* ver. 270.

"We

* IHRE Glossar. voc. *Ordela*.

"We were prepared to grasp the burning irons in our hands, "and to pass through the fire, and to adjure the gods." PACHYMERES has remarked, that μύδρος, or the burning share, was said to be ἅγιος, or holy, because it had been blessed by the priest *.

8. It was believed by some, that by the action of fire, the soul was completely loosed from all its corporeal bonds. TERTULLIAN charges some philosophers with holding, that even after death, certain souls continued in a state of connection with the bodies which they had formerly animated: "Ita argumenta- "tiones emendicant, ut velint credi etiam post mortem quasdam "animas adhærere corporibus †." He asserts, that PLATO was of this opinion. But the passage to which he refers, can only be viewed as a proof that PLATO believed the immortality of the soul. It is the apologue introduced by him in his *Republic*, concerning HERUS the Armenian, who is said to have revived, when he was laid on the funeral-pile, twelve days after he fell in battle ‡. MACROBIUS, however, gives the same account of the doctrine of this philosopher. For, according to him, PLATO asserts that the souls of those who die by violence wander long around

* Ap. DU CANGE, voc. *Ferrum Candens*.
As the Greek word πυρ has been deduced from the Hebrew אוּר, *ur*, properly denoting light, but, as some explain it, including the idea of fire, we might perhaps, with as much reason, suppose that πῦρ was the origin of Latin *pur-us*. On account of the natural subserviency of fire to purification, as well as the ritual use of it with this view, it is possible that the verb *purgo* may have been formed *quasi* πῦρ ἄγω, as originally signifying to lead, or make to pass through the fire. Perhaps the language of the ancient Latin poet NÆVIUS, in the use of the phrase *puriter facere*, may be considered as favourable to this etymon of *purus*; Sequere me, *puriter* volo *facias*, *igne* atque aqua volo hunc accipere. Ap. NON. MARCELL. Gothofred. 775.

† De Anima, p. 501. edit. Paris. 1616.

‡ PLAT. Republic. lib. x. vol. ii. p. 614.

around the body or the place of its sepulture; the soul being more and more fettered to the mortal part: "Exitu autem coac-"to, animam circa corpus magis magisque vinciri. Et revera "ideo sic extortæ animæ diu circa corpus ejusve sepulturam, vel "locum in quo injecta manus est, pervagantur *." "On the "contrary," he says, "those souls, which, during life, are loosed "from corporeal bonds by a philosophical death, are translated "to heaven even while the body exists."

It is not perfectly clear, however, whether MACROBIUS assigns this doctrine to PLATO himself, or to his disciple PLOTINUS, who, he says, carried the principles of his master still farther. In the passage to which MACROBIUS seems immediately to refer, PLATO does not speak of those who die by violence, but in general of men leaving this world under moral contamination †. Elsewhere, indeed, he says, that, besides the immortal soul, the gods have placed in the body of man εἶδος ψυχῆς θνητὸν, "a kind "of mortal soul." This, according to his idea, includes the will and affections ‡.

TERTULLIAN has ascribed the same opinion to DEMOCRITUS; observing that he reasons from the growth of the nails and hair after death ||. But it would seem that here he has rather mistaken the meaning of the language of DEMOCRITUS; as all that we can certainly infer from it is, that he held the doctrine of a future resurrection.

JAMBLICHUS says, that "fire destroyed whatever it found ma-"terial in the sacrifice, purified, and released it from the bonds "of

* MACROB. Somn. Scipion. lib. i. p. 87. edit. Lugd. 1560.

† He expresses his sentiments in the following terms: Ἐμβριθὲς δέ γε, ὦ φίλε, τοῦτο οἴεσθαι χρὴ εἶναι καὶ βαρὺ, καὶ γεῶδες, καὶ ὁρατόν· ὃ δὲ καὶ ἔχουσα ἡ τοιαύτη ψυχὴ, βαρύνεταί τε καὶ ἕλκεται πάλιν εἰς τὸν ὁρατὸν τόπον, φόβῳ τοῦ ἀειδοῦς τε καὶ ἅδου. Phæd. PLAT. Oper. I. p. 81.

‡ PLAT. Timæus, Op. iii. p. 69.

|| TERTULLIAN. ubi sup.

"of matter, and by reason of its purity made things fit for the "fellowship of the gods." "Noster ignis actionem divini ignis imitans, quicquid materiale reperit, in sacrificio, destruit, "et admota purificat, et a vinculis materiæ solvit, ac propter "naturæ puritatem ad deorum communionem idonea fecit *."

According to LUCAN, "the power of the flame carried the "soul into the eternal world." Speaking of the *manes* of the dead, he thus expresses the general belief:

—— Quos ignea virtus
Innocuos vitæ patientes ætheris imi
Fecit, et æternos animam collegit in orbes.
Pharsal. lib. ix. ver. 9.

And OVID says of JULIUS CÆSAR:

Nam Patris Augusti docui mortale fuisse
Corpus; in ætherias Numen abiisse domos.
Pont. lib. iv. Ep. 13.

"The Romans burned the bodies of the dead," says SERVIUS, "ut statim anima in generalitatem, id est, in suam rediret na-"turam †." This corresponds with the account given by SILIUS ITALICUS, when describing the funeral of PAULUS:

—— Recens crepitantibus undique flammis,
Æthereas anima exultans evasit in auras.
Lib. x.

OVID may be viewed as expressing the general persuasion, that the soul was not completely separated from the body, till the latter was consumed on the pyre, when he thus declares the ardour of his friendship:

Spiritus et vacuas prius hic tenuandus in auras
Ibit, et in tepido deseret ossa rogo;
Quam subeant animo meritorum oblivia nostro.
Trist. lib. i. Eleg. 4.

* JAMBLICH. de Myster. Cap. de Ratione Sacrific.

† SERV. in *Æn.* lib. iii.

These incidental intimations of the sentiments of the ancients, coincide with the information which the accurate Eustathius has given us on this subject. " It was a custom," he observes, " among the Greeks to burn their dead, which custom still remains among some northern barbarians: and they " do this to indicate, that the spiritual part of man, being " carried upwards as in a chariot of fire, rises with heavenly " objects, but that the earthly remains behind *."

The Jews, as Grotius has remarked, at least as early as the age of Justin Martyr, had adopted the idea that the souls of the dead lingered about their bodies, and were subject to the power of demons. This opinion, he adds, passed to the Christians, as appears from the reasoning of Justin with the Jew Trypho, concerning the Witch of Endor, who, he thinks, really raised the Prophet Samuel †. Justin, indeed, in another place, appeals to necromancy (νεκυομαντεῖαι), as a proof of the separate existence of the soul ‡.

The same idea, that the soul for a considerable time after death retains some intercourse with the mortal part, is expressed by some later Jewish writers. They hold, that for twelve months after this event, the soul is more or less with the body, hovering over it: and hence some have been induced to go and dwell among the tombs, and inquire at spirits ‖. Others, however, greatly limit this time: " For three days," they say, " the " soul goes to the grave, thinking that the body may return; " but

* Ὅτι ἤθος ἦν Ἕλλησι καίειν τὺς νεκρους. ὃ δὴ καὶ ἐις ἔτι παραμένει τισι τῶν βορειων βαρβάρων. ἐποιουν δὲ τοῦτο ἐκεινοι, πρὸς ἔνδειξιν τοῦ τὸ μὲν θειον του ἀνθρώπου ἀνωφορηθεν ὥσπερ ἐν ὀχηματι τῷ πυρι, προσμιξαι τοῖς ὀυράνοις. τὸ δὲ γηϊνον, κάτω μεῖναι. Eustath. in Iliad. A. ver. 52.

† Grot. in Matt. viii. 28.

‡ Apol. ii. p. 65. ed. Lut. Par. 1615.

‖ Nishmat Chayim, Par. ii. c. 22. p. 81. T. Bab. Beracot, fol. 18, 2.

"but when it sees the figure of the face changed, it goes away, "and leaves it *."

The Greeks and Romans found this doctrine indispensably necessary for the support of a very considerable branch of their system of polytheism. As they ascribed to their heroes or demigods a corporeal substance that was partly immortal, they founded this strange notion on another which was not less absurd. They believed that this partial immortality proceeded from their being in part descended from the gods. Hence, as they supposed that their bodily frame originated partly from mortal, and partly from immortal seed, they persuaded themselves that the fire of the funeral-pile effected a separation between these heterogeneous principles; and that not only the soul, but, as it was sometimes designed, the divine portion of the body, was carried into heaven. Of this extravagant notion we find many vestiges in their writings. HERCULES, it is said, consecrated himself by burning; and was, in consequence of this devotement, admitted into the number of the immortal gods. Hence CALLIMACHUS says of him:

> Ὀυ γὰρ ὅγε Φρυγίῃ περ ὑπο δρυΐ γυῖα θεωθεὶς
> Παύσατ' ἀδηφαγίης.
>
> *Hymn.* in Dian. ver. 159.

It is evident that the poet ascribes immortality to the *members* of the hero. Some have confined the term γυῖα to his soul. But the language will not bear this restriction. For the very design of it is to shew, that HERCULES did not lose his appetite with his deification, but ate as keenly as he had done during his mortal life, and even while he followed the plough; and that, therefore, it was necessary that the Amnisian nymphs

 should

* Bereshit Rabba, § 100. f. 88.

should exercise all possible diligence that they might regularly provide venison for the table of the god. The Emperor JULIAN affirms, that "while HERCULES was a child, his divine body made gradual increase *." The fire, then, was believed to consume the mortal part only; that the soul, with that portion of the corporeal frame which was deemed originally immortal, might be received into the celestial regions. Such was the virtue of the flames, that, like those of the phœnix, they removed the infirmities of age, and communicated eternal youth. Hence THEOCRITUS, who gives the same honour to ALEXANDER the Great, and to PTOLEMY LAGUS, as to HERCULES, says:

> "Οττι σφέων Κρονίδας μελέων ἐξείλετο γῆρας·
>
> *Idyll.* xvii. ver. 24.

Or, as it is rendered by our English translator;

> On each, great JOVE reprieve from age bestow'd,
> And call'd immortal, rais'd into a god.

HERCULES is, by the tragic poet SENECA, made to give the same account of his apotheosis to his mother ALCMENA, in a passage which exhibits the heathen creed on this point more distinctly perhaps than any other now extant.

> —— Quicquid in nobis tui
> Mortale fuerit, ignis evectus tulit;
> Paterna cœlo pars data est, flammis tua.
>
> Hercul. *Œt.* ver. 1966.

This wonderful virtue of the funeral pile, was not, however, entirely confined to demigods. For SCYLLA, who had been slain by HERCULES, was raised from the dead, and rendered immortal

* Ἡρακλῆς δὲ λέγεται παιδίον γενέσθαι, καὶ κατὰ μικρὸν αὐτῷ τὸ σῶμα τὸ θεῖον ἐπιδοῦναι. JULIAN. Imp. Orat. vii. p. 408. ap. SPANHEM. Observ. in CALLIMACH. p. 240.

mortal, by her father PHORCYS; who, by means of the vivifying flames, infused new life into her stiffened limbs:

——῞Ην αὖθις πατὴρ
Σάρκας καταίθων λοφνῖσιν δομήσατε,
Λέπτυνιν ἐ τρέμεσαν ἐδαίαν θέαν.
LYCOPHRON. *Cassandr.* ver. 44. V. POTTER, ii. 208.

It is pretended that the fire, by which the body of HERCULES was consumed, was kindled by the thunderbolt of JOVE; and that, in this manner, the son was called into the presence of his divine parent: Εἶτ' ἐπανήγαγε διὰ τε Κεραυνίε πυρὸς πρὸς ἑαυτὸν *. As the Greeks and Romans forbade the cremation of any who had been killed by lightning †, it is not improbable that this prohibition might originate from the idea, that such persons had been already purified, or consecrated, by the stroke of fire from heaven.

It may be observed in addition, that the rites of apotheosis, or consecration, an honour given to the Emperors of Rome, evidently include the idea that the soul was not completely released from its mortal entanglements, save by the influence of fire. The body, indeed, was previously burned. But an image of the person in wax was substituted, and consumed on a lofty and costly pile, with all possible pomp and solemnity. This pile consisted of four different frames of wood, gradually decreasing in their dimensions, and placed one above another. When the fire had nearly reached the fourth or highest frame, an eagle was let loose from it, "which, ascending with the "flames towards the skies, was supposed to carry the prince's "soul to heaven ‡." On some of the coins, struck in memory

* JULIAN. Imp. Orat. vii. p. 408.; ap. SPANHEM. ut sup. p. 241.

† V. GUTHER. de Jure Manium, lib. i. c. 3.

‡ V. HERODIAN. Hist. lib. iv.

ry of consecrations, the emperor appears carried aloft by an eagle *. Or, perhaps, the figure here exhibited is rather to be viewed as the emblem of his soul.

9. The soul itself was thus supposed to be purified from the contamination which it had contracted in its embodied state. The ancient Gymnosophists of India, of whom the *Brachmans*, now called *Bramins*, formed one sect, and the *Germanes*, *Hermanes*, or *Sermanes*, another, were wont to burn themselves alive †; although, in our day, they require this sacrifice of their wives only. That they ascribed some peculiar virtue to fire as thus applied, might be inferred from the language of that Indian, who, when he cast himself into the flaming pile at Athens, said to the astonished spectators, "Thus I make my-"self immortal ‡." But perhaps the inference is confirmed by the account which Porphyry gives of the reason of this act of suicide. Having observed, that they who are about to devote themselves in this manner, first coolly receive from those around them the commissions which they wish them to carry to their friends in the other world; he subjoins, "They cast "their body into the fire, that they may separate the soul from "it in a state of the greatest possible purity." This at least seems to be the natural meaning of the words of Porphyry; *πυρὶ τὸ σῶμα παραδόντες, ὅπως δὴ καθαρωτάτην ἀποκρίνωσι τοῦ σώματος τὴν ψυχὴν* ||.

Lucian, when speaking of the self-devotement of Hercules, which he attributes to mere ostentation of fortitude in suffering,

* V. Havercamp. Nummophylac. Reg. Christin. p. 100, 101.

† Θανατῳ δὲ ἑαυτὺς ἀποδιδόασι κενῷ, καθάπερ καὶ οἱ τῶν Ινδῶν γυμνοσοφισαὶ ματαίῳ πυρί. Clem. Alexandr. Stromat. Lib. iv. p. 351.

‡ Nic. Damascen. V. Hydriotaph. p. 3.

|| De Abstinentia, lib. iv. sect. 18.

ing, introduces the Brachmans, and refers to the history of CALANUS, who, leaving his own society, followed ALEXANDER the Great from India. " The Brachmans," he says, " do not leap " into the fire like ONESICRITUS, the Governor of ALEXANDER, " who, it is related, when he saw CALANUS burning, flung him-" self into the flames; but after they have erected the funeral-" pile, standing immoveable beside it, patiently allow them-" selves to be accosted, then ascending it with dignity, are " consumed, not shrinking in the slightest degree from the ap-" proach of the fire *." But, as it was the design of LUCIAN to ridicule the philosophers, he assigns no other reason for this conduct than the love of vainglory. To the same motive does he ascribe the act of PEREGRINUS, a Stoic, or, as he says, a Cynic philosopher, who devoted himself to the flames.

CELSUS the Epicurean, however, as quoted by ORIGEN, viewed this suicidal act in a more favourable light. His testimony corresponds with that of PORPHYRY. His language intimates, that, what virtue soever might be ascribed to the flame of the funeral-pile, in ordinary circumstances, the effect was supposed to be far greater, if any one consigned himself to it alive. ORIGEN thus expresses his sentiments: " CELSUS says, that it is im-" pious to violate the laws of our country in regard to the bles-" sed termination of life (as it is accounted) by the funeral-" pile, into which those who voluntarily cast themselves are " perfectly purified in their aberration from life †."

QUINTILIAN ascribes the same purifying efficacy to the funeral-pile. Speaking of the soul, he says; " Quoties humani pecto-" ris

* LUCIAN. de Morte Peregrin. Oper. ii. p. 576.

* Ἡμῖν ὁ Κέλσος, πῶς ὀυχ ὅσιον παραλύειν νόμους πατρίους—περὶ τοῦ μακάριον ἐιναι τυγχόνη τὸν βίον ἐξελθεῖν. ἢ πάντως καθαίρεσθαι τοὺς ἑαυτοὺς παραδιδόντας τῷ πυρὶ, καὶ τῇ διὰ πυρὸς ἀπαλλαγῇ τῇ ἀπο τοῦ βίου. ORIGEN. cont. CELS. Lib. v. p. 249. " Καθαίρεισθαι," says the learned STUCKIUS, " idem quod λυμαίνειν; expiare, resecrare, purgare, expurgare, purificare." Sacr. Sacrific. Descript. p. 123.

" ris carcerem effugerit, et exonerata membris mortalibus levi " se igne lustraverit, petere sedes inter astra *."

SERVIUS, when explaining the language of VIRGIL,

> —— Aliæ panduntur inanes
> Suspensæ ad ventos: aliis sub gurgite vasto
> Infectum eluitur scelus: aut exuritur igni——

remarks, that he " speaks poetically concerning the purgation " of souls; for he alludes to what the philosophers said." He then proceeds to shew, that there was a threefold purification of man, by earth, by water, and by air; that the earthy purification denoted that which was made by fire, which has its origin from earth; and that this was necessary for those who had indulged in sensual enjoyments †.

As it was accounted unlawful for Christians to burn their dead, TERTULLIAN assigns as one reason for their rejection of this practice, that they had already received the benefit of a purification far superior. " Et cremabitur," he inquires, " ex disciplina castrensi Christianus, cui cremare non licuit, cui " Christus merita ignis indulsit? ‡"

It is well known that the Platonists denominated the end of the present state of this world ἀναπύρωσις, as believing that it should be purified and refined by fire; and that to this change the Stoics gave the name of ἐκπύρωσις. As it is equally certain that many of the dogmas of the Oriental, of the Platonic, and

* QUINTILIAN. Declam. x.

† Loquitur quidem poëtice de purgatione animarum: tangit tamen quod philosophi dicunt. Nam triplex est hominis purgatio. Aut in terra purgantur: quæ nimis oppressæ sordide fuerunt, deditæ scilicet corporalibus blandimentis, enim transeunt in corpora terrena; et hæc igni dicuntur purgari. Ignis enim ex terra est, quo exuruntur omnia, nam cœlestis nihil perurit. SERV. in *Æneid.* vi. ver. 742.

‡ TERTULLIAN. de Corona Militis, p. 292.

and of the Stoic, philosophy were early incorporated into the Christian system; it has been asserted by several learned writers, that the Popish doctrine of a middle state, or of the purification of souls by fire, was borrowed from that of the ἐκπύρωσις. The doctrine of purgatory, indeed, is nearly allied to the ideas which the gentiles entertained concerning the efficacy of fire in preparing the soul for the abodes of Elysium.

It has also been observed, that the sacrifices which the Greeks denominated τελεταὶ, were offered with the same design. Some have supposed that these sacrifices for the dead received this name, because, being the most sumptuous of all sacrifices, the greatest part of them was consumed *. But there is another interpretation of the term, which is more probable. They seem to have been thus denominated, as *perfecting* whatever had been deficient in the merit of the dead, and as securing their liberation from suffering in the eternal state †.

10. I shall only further remark, that this igneous purification was intimately connected with sacrifice. Though, as has already been observed, we cannot view this practice as originating from the tenets of any particular school; sufficient proof has been brought to shew, that it was viewed as a solemn act of religion: and it may naturally enough be supposed, that the philosophers who considered fire as the principle of all things, would take advantage of a custom which was pretty general in their times, as affording an argument that seemed to lend its aid to their peculiar system. As this custom was observed, not merely as a religious rite, but as an important means of purification; as it has been so generally diffused, although doubtless abhorrent from the feelings of humanity; I have at times been

 inclined

* SUID. in voc.

† V. GALE's Court of the Gentiles, Part III. B. ii. c. 2. § 11.

inclined to think, that, at an early age, the cremation of the body might have been immediately meant as an act of expiatory sacrifice. Of this, however, it must be acknowledged, we have no direct evidence. But the want of this evidence is not a decisive proof that the conjecture is totally unfounded. The origin of the practice itself being buried in the obscurity of very remote ages, almost beyond the period of fabulous history; it is by no means surprising, that we should be as much at a loss in regard to the primary design. On a subject of this kind, even the feeble voice of conjecture ought not to be withheld, as no other can be heard; nor should it be totally disregarded, because at times mere conjecture has eventually led to the discovery of truth.

It is highly probable, that this custom had its rise in the distant regions of Scythia, if not in Hindostan. As we learn from the earliest accounts of the inhabitants of this interesting peninsula, which have reached our times, that even when these were written, they not only burnt the dead, but that the living often devoted themselves to the flames; it may afford ground for conjecture that the latter practice preceded the former. Those who viewed it as a signal act of piety for a man to cast himself into the funeral-pile, and as a complete atonement for all the transgressions of his past life, though they could not rival him in intrepidity, might, like those who affect liberality in their latter wills, wish to imitate him as far as possible, by devoting the mortal part to the fire after the extinction of life.

It may perhaps be supposed, that this act of self-dedication to the flames was originally meant as a consecration to the sun. For, though the Persian fire-worshippers objected to cremation as a profanation of their deity, we are not warranted to conclude, that all who concurred with them in worship entertained the same idea. While we know that some of the ancient

Brachmans

Brachmans devoted themselves to a fiery death, we are assured by STEPHANUS BYZANTINUS, from HIEROCLES, that they were chiefly consecrated to the sun *. ÆLIAN expressly declares, that CALANUS the Gymnosophist, after he had ascended the funeral-pile on which he devoted himself, adored the sun which shone on him; and that this was indeed the signal which he had agreed to give to the Macedonians, that they should set fire to the pile †. LUCIAN gives a similar account of the death of PEREGRINUS, who, although a Parian by birth, seems to have imitated the customs of the Gymnosophists. The pile being kindled, "he called for incense; which when he had received "from some one, he cast into the fire. Then looking stedfast-"ly towards the meridian, (for the meridian was concerned in "this tragedy), he said, O! ye maternal and paternal demons, "receive me graciously! Having said this, he threw himself "into the flames." He subjoins, "It has been lately report-"ed, that, as he ascended the pyre, he saluted the rising sun, "as it is said the Brachmans are wont to do ‡." It merits observation, that the Bramins of our own time daily pay their devoirs to the sun; not considering him, they pretend, as the proper object of worship, but as the visible representative of the Supreme Being. Incense, I am assured on good authority, is uniformly used by them, in the manner above described, in the

 inhuman

* Βραχμάνων ἰδεῖν φῦλον ἀνδρῶν, φιλοσόφων καὶ θεοῖς φίλων ἡλίῳ δὲ μάλιστα καθωσιωμένων. STEPH. BYZANT. VOC. Βραχμᾶνες.

† Καὶ ὁ μὲν ἥλιος αὐτον προσέβαλλεν: ὁ δὲ αὐτὸν προσεύνει: καὶ τῦτο ἦν τὸ σύνθημα εἰς τὸ ἐξάπλειν τὴν πυρὰν τοῖς Μακεδόσι. ÆLIAN. Var. Hist. Lib. v. c. 6.

‡ Εἶτα ᾔτει λιβανωτὸν ὡς ἐπιβάλοι ἐπὶ τὸ πῦρ. καὶ ἀναδόντος τινὸς, ἐπέβαλε τε, καὶ εἶπην ἐς τὴν μεσημβρίαν ἀποβλέπων, καὶ γὰρ δὲ τῦτο πρὸς τὴν τραγῳδιαν ἦν ἡ μεσημβρία, δαίμονες μητρῷοι, καὶ πατρῷοι, δέξασθέ με εὐμενεῖς.—Καὶ γὰρ δὲ τόδε τῇ προτεραίᾳ διεδέδοτο, ὡς πρὸς ἀνίσχοντα τὸν ἥλιον ἀσπασαμενος, ὥσπερ ἀμέλει δε τῦς Βραχμὰνας φασι ποιεῖν, ἐπιβήσεσθαι τῆς πυρᾶς. LUCIAN. De Morte PEREGRIN. Oper. ii. p. 584,—586.

inhuman act of burning the living with the dead. This act is invariably performed in the vicinity of a river. For all the ashes of the dead are collected, and completely dispersed on the water; as if they accounted both these elements, which other nations used for purification, necessary for perfecting this horrid consecration.

There can be no doubt that the Greeks and Romans also worshipped fire, under the names of Eϛια and *Vesta*. It may be remarked by the way, that as the Latin name of this deity is evidently from the Greek, some learned writers have with great probability traced the Greek word to that country in which, it has been supposed, fire-worship had its origin. The Chaldaic term *Esth* signifies fire, synonymous with Hebrew אשה, *ashch* *.

Whether cremation was originally meant as a sacrifice or not, it cannot be denied that a variety of circumstances were conjoined with it, which had the closest connexion with this act of religion.

The funeral-pile was erected in the form of an altar. Hence the language of VIRGIL, when describing the obsequies of MISENUS;

——— Aramque sepulchri
Congerere arboribus, cæloque educere certant.
Æn. lib. vi.

Cremation was accompanied with the oblation of victims. Originally the blood of captives was shed. ACHILLES sacrificed twelve gallant Trojans in honour of his friend PATROCLUS †. Gladiators were afterwards substituted for captives. These were

* V. STUCKII Sacr. p. 151.; PIER. Hieroglyph. fol. 135, b.; BOCHART. de *Æn.* p. 13.

† Iliad. ψ ver. 175.

were by the Romans called *Bustuarii*, because their blood was shed before the *bustum*, which was the designation given to the funeral-pile, after the body was *combustum*, or burnt; or, as others say, quasi *bene ustum*, thoroughly burnt or consumed. For it bore the name of *rogus*, while the fire continued to burn; because, as SERVIUS explains it, during this operation the attendants continued *rogare*, to call upon or invoke the *manes* of their departed friend. They sometimes sacrificed beasts, as oxen, swine, &c. which they threw into the blazing pile.

> Multa boum circa mactantur corpora morti,
> Setigerasque sues, raptasque ex omnibus agris
> In flammam jugulant pecudes.
>
> *Æn.* lib. xi.

It must be acknowledged, however, that while the ancients, in some passages, unquestionably speak of these offerings as made to the *manes*, or ghost, of the person whose funeral was celebrated, in others their language can apply only to the *Dii Manes*, or infernal gods. This inconsistency causes considerable difficulty in attempting to form a judgment with respect to their proper design in these oblations.

When the body was consumed, they extinguished the fire by pouring wine upon it. This is said to have been done, that they might more easily collect the bones and ashes. But even this has much the appearance of a sacrifical act, and may originally have been meant as a libation. Water, because of its purity, might otherwise have been preferred for extinguishing the flames. From the manner in which HOMER describes the employment of ACHILLES, while watching the flaming pile of PATROCLUS by night, it would seem that he continued to pour wine on the ground, as a libation to the *manes* of his friend:

Οἶνον

Οἶνον ἀφυσσόμενος χαμάδις χέε, δεῦε δὲ γαῖαν,
Ψυχὴν κικλήσκων Πατροκλῆος δειλοῖο.
Iliad. Ψ. ver. 220.

Plato informs us, that the ancient Greeks hired female mourners, whose office it was to bewail the dead, and to make libations *.

It may be also observed, that they evidently wished that the wine, used on this occasion, should as nearly as possible resemble blood. Thus, in the account given of the funeral of Hector, we find it expressly mentioned that the wine with which the remains of the fire were extinguished, was dark-coloured.

Πρῶτον μὲν κατὰ πυρκαϊὴν σβέσαν αἴθοπι οἴνῳ
Πᾶσαν ὁπόσσον ἐπέσχε πυρὸς μένος.———
Ibid. Ω. ver. 791.

Now, this is the very language which the same illustrious poet had employed to denote the libation by Chryses, the priest of Apollo, on the joints of the hecatomb, which he offered as an expiatory sacrifice for the Greeks.

Καῖε δ' ἐπὶ σχίζης ὁ γέρων, ἐπὶ δ' αἴθοπα οἶνον.
Ibid. A. ver. 462.

I have already adverted to the Indian practice of casting frankincense on the funeral fire. As the use of incense has from time immemorial been an established rite in sacrifical worship, it appears that it was not unknown to the Greeks and Romans, in celebrating the obsequies of the dead. Kirchman has

* Ἱερεῖα τε προσφάττοντες πρὸ τῆς ἐκφορᾶς τοῦ νεκροῦ, καὶ ἐγχυτρίστριας μεταπεμπόμενοι. Plat. *Minoe*, Oper. ii. 315. Kirchman supplies some observations on this singular rite. De Funer. p. 370.

has remarked, that he finds incense mentioned among other spices, which were thrown into the funeral-pile. The great expence in multiplying these might latterly proceed from mere ostentation. In some instances, they might be merely meant to overpower the fetid odour arising from the act of cremation. But it may be supposed, that the use of incense had originally a sacrifical signification. LUCAN, when describing the funeral of POMPEY, mentions this as the only odoriferous substance that was burnt with the body *.

Non pretiosa petit cumulato thure sepulchra
POMPEIUS, Fortuna, tuus.——

Pharsal. lib. viii.

Without particularising the games celebrated at funerals, or the feasts connected with them, which were on other occasions accompaniments of sacrifice, I shall only add, that besides the pyre, which had the form of an altar, another altar was erected, after cremation, immediately before the sepulchre. This received the name of *acerra;* and is by SERVIUS expressly distinguished from the funeral pile †.

* KIRCHMAN. De Funeribus Romanorum, p. 226.

† In *Æn.* lib. vi.

VI.

[illegible]

VI. *Additional Communications respecting the Blind and Deaf Boy,* JAMES MITCHELL. *By* JOHN GORDON, M. D. F. R. S. EDIN.

(Read Nov. 20. 1815.)

THE following circumstances respecting the Blind and Deaf Boy, JAMES MITCHELL, have come to my knowledge since the publication of Professor STEWART'S Memoir; and I doubt not but the Society will think them worthy of being recorded. They are derived from the most accurate and authentic of all sources, the boy's sister, Miss MITCHELL.

In the month of April 1814, Mr PARKER, an English gentleman, (distinguished, as I have since learned, for his active benevolence,) did me the honour to wait upon me, to communicate a plan for the instruction of young MITCHELL, which had some time before occurred to him, and which he was very desirous should be put to the test of experiment. This method seemed in no respect inconsistent with those principles which in all circumstances appear to regulate the acquisition of language; and I therefore expressed my willingness, to promote, by every means in my power, the object which Mr PARKER had in view.

Mr Parker, accordingly, took the trouble of getting an alphabet cut in wood; the letters of which were *in relief*, and in separate pieces, each about an inch long. He provided, also, two other alphabets, one of pasteboard, and the other of metal, with the letters of the same size, and detached, as in the former. These alphabets I transmitted to Miss Mitchell on the 2d of June; informing her, at same time, of the purpose for which they were intended; and expressing my anxiety, that she would lose no time in giving the plan proposed a fair trial. Particular directions for the use of the alphabets were drawn up by Mr Parker; and I took the liberty of adding only a few general hints; being well aware how unnecessary it was to go into the *minutiæ*, when addressing so judicious a preceptor.

The outline of the plan was simply this: The name of any familiar object being chosen, such as *egg*, *bread*, *sugar*, *arm*, &c., the letters forming the word were to be put together by Miss Mitchell, exactly as they are arranged in print. Mitchell was then to be made to touch, first the object, and then this word, in immediate succession, as often as possible; so as to form a close association in his mind, between the *thing* and its *tangible name*. It was left entirely to Miss Mitchell's judgment, to employ such means as she deemed best, for securing her brother's perseverance in the task for such a length of time, as might enable him to perceive its object: And in the event of this primary and fundamental step being gained, the experiment was to be prosecuted, according to similar principles.

The following is a copy of a letter which I received from Miss Mitchell, dated Nairn, the 30th June.

"I have been from home during these last ten days, which has prevented my sooner acknowledging the receipt of your favour

favour of the 2d instant, accompanying a parcel from Mr Parker, to whom we consider ourselves very much indebted for the interest he takes in my brother. Have the goodness to make offer of our warmest acknowledgements to him, and say that I shall, with much pleasure, avail myself of the liberty he allows me, of informing him of the progress made, in the plan he has so ably sketched for my brother's instruction, which I mean to attempt immediately, and shall not be easily discouraged by not succeeding at first. If I can once interest my brother, by affording him any gratification, in my communications by the means of letters, I shall have very great hopes of success. I shall first try the pasteboard letters, as I think them less likely to distract the attention than the wooden ones.

"My brother was twice at Ardclach of late. The first time, he probably found his way there by chance, as he had not been at Ardclach before, since he came to reside at Nairn; but the second time, he must have gone intentionally, as there was only one day between the two visits. I have seen Mrs Macbean since these visits, and inquired particularly respecting his conduct. The first day, after having taken some refreshment, he went through the different apartments of the manse, examining, by touch, the furniture, &c. and seemed to miss a closet-door, which had been shut up after we left the place. He did not betray any particular emotion, upon thus visiting, for the first time, a house which had been for so many years his home; but when he had satisfied his curiosity, seemed anxious to get away, and returned directly to Nairn. On the second visit, he found workmen employed in taking down the kitchen, (a part of the repair which the manse was then undergoing,) and after standing some time, evidently very much displeased at the work of destruction which was going forward, he came away in very bad humour, and could not be prevailed on to return and

go into the house. He did not discover the least wish to visit the church-yard either time.

"I had an opportunity of observing his conduct with regard to a dead body very lately;—the body of an old gentleman, a near neighbour of ours, and one who had been very kind to him, frequently indulging him with a pipe and tobacco, his favourite gratification. I brought him to the room where the body was laid, and allowed him to feel it, which he did very willingly, not shrinking as upon a former occasion, but seemingly rather anxious to examine it. When he had done so, he stood for a few seconds, rather thoughtful; and this was followed by a smile, with, I thought, something of wildness in the expression of the countenance. He then came away very willingly with me, but not before he shewed that he recognised the person, and was sensible of the situation. This he did, by making his usual sign for smoking, and by putting his hand to the ground, his sign for interment. He discovered a wish to learn when the ceremony was to take place, (by a slight inclination of his head to one side,) which I endeavoured to inform him of; and he kept constantly in the way until it was over; frequently going to the apartment where the body was kept; but without discovering sorrow, further than now and then appearing rather thoughtful.

"These particulars are perhaps of little consequence; but from the great interest you have always taken in my brother, I think it right to mention them. And, with best respects, I am," &c.

The following letter, dated Nairn, 31st October 1814, is from Miss Mitchell to Mr Parker, and communicates the complete failure of her attempts to educate her brother by means of the *tangible alphabet*.

"Sir,

"Sir,

"I have from time to time deferred writing you, from a desire rather than from a hope, of being able to give you favourable accounts of the result of my endeavours to instruct my brother, by the means of letters; and regret excessively being obliged to state, that I have completely failed in putting into effect the plan you so ably sketched out for me, principally, and, indeed, I may say wholly, from my brother's wanting the necessary habits of application. With my first attempts he seemed rather amused, but afterwards appeared teazed, and got into bad humour, and, without risking the loss of the little power I have over him, I could not persist in irritating him, being sensible that I only retain it from having recourse to it seldom, and using it sparingly. How much I am grieved, at thus being obliged to relinquish a plan from which so much benefit might have been derived, I cannot say; my only consolation is, that it is not from any want of exertion on my part. Had any such plan been commenced with him in infancy, or at an early period, and steadily persevered in, I doubt not but it might have had the wished for success; but now, when his habits are formed, and his passions strong, I much fear there is little chance of any thing being done; at least if there is any thing done, it must be by some person who has more the power of controlling him than I now have. In short, I am unable to make it sufficiently interesting to be a source of amusement to him, and, as a task, he will not apply to it. Nor is it (however much to be regretted) astonishing that he will not, accustomed, as he has always been, to follow his own immediate gratification only, and dispose of his time as inclination leads him.—Allow me now to make offer of our warmest acknowledgments for the benevolent interest you have taken in this affair, and to assure you, that it will afford us much happiness to be

be at any time able to show, how sensible we are of your goodness. And I have the honour to be," &c.

In the month of December 1815, I had the pleasure of receiving a letter from Miss Mitchell, from which the following is an extract.

"I cannot say that my brother is making any very visible progress in knowledge; but he had a severe illness, about ten or twelve months ago, which rather placed him in a new point of view, and I shall endeavour to mention what we considered as most striking in his conduct. In the first place, he seemed very apprehensive of dying, at least we could not otherwise account for many of his actions, than by supposing they proceeded from a fear of death. Although reduced to that state of debility that he could not move without two people supporting him, we never could prevail with him to lie a single day in bed; he literally watched the first appearance of dawn, and insisted on being dressed immediately, thinking, probably, that he would not die out of bed. Any thing *white*, too, he could not bear to see near his bed, or even in the room with him. Several times, by accident, something white was thrown across the foot of his bed, and he appeared most unhappy until it was removed, attempting most eagerly to grasp it himself, before we discovered the source of his uneasiness. In the same way, too, when any linens were put to the fire to air by him, he was in the greatest possible distress until they were taken out of his sight; and this, when there was not any glare of light that could affect him; it, therefore, must have been some idea connected with them that distressed him; and from his having always seen dead bodies laid out in white, we could only attribute his evident dislike, to his associating the idea of death and this appearance together. He took a particular fancy

cy to a sister of my father's, who was here at that time, insisting on her sitting constantly by him, (probably from finding her kind and attentive to him). But I chanced to be taken ill before he was quite recovered; and after my being attacked, he would not allow her to sit down near him, but always signed to her to go *up stairs* where I was, and was not satisfied until he made good his point. This is, perhaps, the most decided instance of affection and consideration for others, he has ever shewn. He, once or twice, discovered a wish to get up stairs himself, and, upon being brought up, seemed quite satisfied when I patted him, and shook hands with him."

VII.

VII. *On the Education of* JAMES MITCHELL, *the young Man born Blind and Deaf. By* HENRY DEWAR, M. D. F. R. S. EDIN.

(Read 4th December 1815.*)*

THE practicability of instructing in any kind of language a person blind and deaf from infancy seems not to have occurred to the friends of JAMES MITCHELL till suggested by Dr GORDON, whose ideas on the subject are contained in Professor STEWART's account published in the Transactions of this Society *. An attempt has been since made to put in practice a plan for the same purpose, proposed by Mr PARKER, an English gentleman who resided for a short time in Scotland. This consisted in accustoming him to handle the letters of the alphabet, formed of pieces of wood or pasteboard, when placed together so as to compose different words significant of tangible objects, and making him handle the objects in order to learn their meaning. This, however, failed, in consequence of the unwillingness of the pupil to submit to the necessary application. For the details, I refer to the account lately laid before the Society by Dr GORDON †, and particularly to Miss MITCHELL's letter to Mr PARKER, dated the 31st of October 1814, describing the result of her endea-

vours,

* See Vol. vii. p. 70. † See the preceding Paper in this Volume.

vours, and containing her own judicious observations on the subject.

The object of the present paper is, to describe a plan which seems to be exempt from the obstacles attached to that which has been unsuccessfully tried. It consists chiefly in teaching the words, and enabling the pupil to use them without taking the trouble of combining the letters. This is to be done by providing him, in the first instance, with entire words ready formed in a permanent state, shewing him their meaning, teaching him to distinguish them by simple means, and leaving him to find out, at a comparatively late stage, by his own deliberate observations on their constituent parts, the forms of the letters, and the use that is made of their various combinations.

The manufacture of words may be conducted in the following manner. A few sets of metallic types may be procured, with the letters hollow and reversed, of a size sufficient to serve as moulds for casting letters in relief, the form of which can be distinctly perceived by the fingers. A diameter of three-fourths of an inch will probably serve the purpose. That the signs taught may not be unnecessarily multiplied, the use of capitals should be dispensed with. The letters should resemble written characters, that he may understand those which are familiarly used in the communications of other persons; but they should not run into one another; they should even be placed at a greater distance than is done in printed books, that he may handle each letter in all directions with facility, and thus learn the sooner to analyse his words. The metallic types may be placed in the order corresponding to the orthography of each word, in the bottom of a groove, a little deeper than the thickness of the types, and of a diameter just sufficient to admit them. The groove will then serve as a mould, in which the word may be formed of plaster of Paris, softened sealing-wax, flour-paste, or any other convenient plastic substance.

In this manner the names of the objects most familiar to him may be procured in the form of labels resembling paper-folders. That he may receive a just idea of the positions of the letters without future correction, the lower margins of the labels may be distinguished by a slight ledge, and the words always given him in the right position. The words should be laid aside in shelves or pigeon-holes, to which he has access. All those beginning with the same letter should be contained in a separate division. A more minute arrangement would probably, in the first instance, be rather troublesome to him. The advantages of the arrangement made ought rather to be left for himself to discover, than industriously inculcated.

The mode in which he should be taught the meaning of the words consists in making him handle an object, and, at the same time, the slip containing its name. This is easily done with such words as *coat*, *shoe*, *stocking*, *water*, *milk*, *bread*, *stone*, *wall*, *tree*, *wood*, and *knife*.

If he is left entirely to himself in distinguishing the words, his mind will fix on any circumstance that happens to occur to him, such as their comparative length, or the form and situation of particular letters which strike his fancy. This spontaneous process would have the advantage of being free from any proceeding unnecessarily dictatorial. But a little direction, scarcely amounting to a greater degree of it than is implied in giving him the words, may be employed, as conducive to regularity. His finger, for example, may be guided along the first and second letter of each word, in the direction in which the pen moves in writing. It will not be necessary to turn his attention so particularly to the succeeding letters, till such time as he is to be made acquainted with a plurality of words coinciding in the first two. He may be made, for example, to trace the *h* and

the *o* in the word *horse*, immediately before being brought to that animal. His attention will not require to be turned to the *r* till he is to be made acquainted with such words as *house*, *home*, and *hot*. In like manner, when he is to be made acquainted with the word *horn*, coinciding with *horse* in the first three letters, his attention will be directed to the fourth. The first three will probably by that time be so familiar to his tact as to require but little tracing, on the same principle on which other persons learn by habit to read syllables and words without spelling.

He ought to receive in the very beginning a considerable variety of words; for, though he is not likely to learn many thoroughly at once, he will thus have a chance of sooner understanding the design with which they are given. A few should be more particularly forced on his attention; and, as it is by distinguishing objects which are nearly allied that he will soonest perceive the utility of language, it would be advisable to direct his mind, in the first place, to a single *class* of objects, such as the articles of food and drink, the names of which might be given to him very fully. When he sits at table, the name of each article may be placed beside it, and opportunities thus allowed him to practise this species of association. Significant words may next be appended to other familiar objects which admit of it. The frequent repetition of these associations is probably a better method of producing an impression on the memory, than the exaction of a task which requires exertion of mind. A deep interest may be excited by making him handle the name of a favourite object, such as the word *milk* or *bread*, a few minutes before the object itself is given to him. The association will thus be aided by the operation of encouraging hope or pleasing curiosity. This will be both a milder and more effectual expedient, than to make his proficiency the condition of

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the gratification of his wishes, by withholding an object till such time as he can present his teacher with its name. This last should not be attempted before there is reason to think that he has acquired some patience of temper, at least not in his present situation, where he is subjected to no systematic authority; nor should it in any case be practised till the association has been repeated with sufficient frequency to entitle those around him to expect that he will remember it. It is, however, by the names of the most favourite objects that his interest will be soonest secured, and it will always be proper to shew peculiar promptitude in understanding and gratifying his wishes when he is able to make them known by producing the name of the object which he wants. A little experience of the advantage arising in this respect from language will create a desire to make similar communications respecting objects of all kinds.

Some would, perhaps, from their opinions on the theory of grammar, reckon it most correct to teach him in the first instance substantives alone. But it will be attended with great advantage to teach the adjectives almost equally soon. They may be learned with the same facility; and they will prove to him interesting, as affording expressions for his more general, and what have been sometimes called *moveable* ideas. They will also assist in making him perceive in less time the intentions of the lessons given to him. He may be made to touch one vessel, and at the same time the words *warm water*, and another vessel while he touches the words *cold water;* and in the same manner proceed to learn the meaning of such combinations as *warm tea, cold tea, sweet tea, weak tea, strong tea; heavy stone, light stone, large stone, small stone*, and others of similar application. It would be advisable to give him a separate set of shelves or pigeon-holes for containing the adjectives, or to distinguish them by some peculiar mark.

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He may afterwards learn words of more general import, and those which characterise temporary relations. One stone may be placed on his knee, or close to him on a table, while he handles the words *this stone*; another at a greater distance, while he handles the words *that stone.* He may also learn various gradations of qualities, as *cold*, *temperate*, *warm*, *hot*, *very hot.* His lively and intelligent turn of mind affords a hope that words of all kinds may be effectually explained to him by well-marked and varied exemplifications of their use.

The chief difficulty will be, to make him understand the intentions of his friends in following these measures, and to impress him with their utility. All the difference, however, that exists between him and other persons, in this particular, consists in the smaller number of lessons that can be given to him in the same space of time. No essential medium of communication is wanting. It is only by observing a number of motions, and hearing a number of vocal sounds in themselves unmeaning, and perceiving the association of them with the occasions on which they habitually occur, that children perceive the intention of human language. They enjoy the great advantage of having the materials of it, in their various connections, perpetually offered to their notice. James Mitchell is entirely dependent on the instructions given to him intentionally, and with considerable labour, by other persons; and his progress, in the first instance, must be proportionally slower, as the number of instructive impressions made on him in any interval of time must be smaller. Some advantage in compensation of this will be derived from the greater maturity of his natural powers. Yet we must be aware that some disadvantage will arise from the comparatively fixed state of his habits. If the first difficulties are once surmounted, there is every probability that he will perceive the utility of language in opening

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to him new sources of agreeable knowledge, and will then make voluntary exertions to instruct himself by engaging in solitary lessons, or by soliciting in particular instances information from those around him.

It is after he has, on this plan, made considerable progress, that success may be expected in teaching him the useof the separate letters. For this purpose, letters formed with the same types may be given to him in a drawer of twenty-four divisions, in alphabetical order. By means of these, his familiar words may be frequently formed for him in his presence, and he may gradually learn to form them for himself. There will, however, be no propriety in urging this part of instruction with much earnestness. If he shows himself in any degree reluctant to it, his teacher may be content with giving him farther instructions in prosecution of the first part of the plan. In addition to such instructions, it will be sufficient to give him, in the first instance, an opportunity of forming words for himself when he is inclined. By being first made a complete master in one department, he will be prepared to make more rapid progress in another.

Till he has made considerable proficiency in the knowledge of entire words, and acquired a relish for language, any further expedients are comparatively of little interest. But, anticipating success in this part of the undertaking, the execution of which must be regarded as the most difficult, I proceed to describe some subsequent expedients. These will evidently admit of being greatly varied according to unforeseen circumstances.

If it is found that, after having learned the use of entire words, he cannot be easily taught to combine the letters, it will be proper to increase his stock of words. Other parts of speech besides substantives and adjectives may now be given

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to him. Verbs of motion and of posture will be easily taught, and rendered interesting by being associated with the motions and postures which they denote. Such are the words *walk*, *run*, *strike*, *break*, *eat*, *drink*, *lie*, *sleep*. The words *is* and *not* will prove extensively useful, by enabling him to connect, in the form of sentences, his substantives and his adjectives. He should also be provided with some significant terminations, particularly the letter *s* and the syllable *es*, which may, on proper occasions, be attached by some easy expedient to the labels containing his other words. It will be very easy to show him the difference between *stone* and *stones*, *cup* and *cups*, *dish* and *dishes*. The use of the terminations *ed* and *ing*, and a variety of others, will naturally follow in the order of their relative importance.

If he can be brought thus far, the rudiments of connected discourse will gradually accumulate; but the process requires to be conducted by a person who has sufficient discrimination to introduce no words except such as correspond to the limited knowledge admitted by the senses of which he is possessed. Such short sentences as the following may first be taught him: *This water is warm; that water is cold; it rains; it blows hard; go to the house; take off your clothes; we must go to sleep.*

After he is well advanced, he may be furnished with some sort of horn-book to facilitate his communications. Perhaps the best form for this purpose would be a glove, with the letters in relief attached to it; the vowels being placed on the tips, and the consonants under them, in alphabetical order, on the bones and joints of the fingers. This will insensibly conduct him to the practice of a convenient dactylology, and he will in no long time be able to converse with his friends by the fingers, without the use of the glove. With a view to this acquisition,

acquisition, his alphabet may, from the very beginning, be arranged in lines corresponding to the following order:

a	*e*	*i*	*o*	*u*
b	*f*	*j*	*p*	*v*
c	*g*	*k*	*q*	*w*
d	*h*	*l*	*r*	*x*
		m	*s*	*y*
		n	*t*	*z*

A subsequent stage of instruction will be, to send messages to him, which may be impressed on surfaces of wax, and induce him to return answers, or to send other messages.

Perhaps the art invented by Mons. Haüy for the use of the blind may be found convenient for such purposes, that of impressing the letters with direct types on strong paper in a moistened state, so as to give tangible letters in bold relief on the opposite side. Not having seen any of the articles produced by this manufacture, I cannot give an opinion on the extent of its utility as applied to the present object; but hard casts are probably better fitted to insure success at an early stage of the process, as they are not liable to be injured by frequent handling.

The teaching of the numbers is a separate object, but comparatively easy, and may be conducted in such a manner as to prove amusing both to him and to his teacher. It should be done entirely by means of our common ciphers, without the use of words. To a person in his circumstances the names of the numbers in letters would be of no use; and it is easy for every person who afterwards converses with him to employ ciphers exclusively. This rule, with the omission of the use of capital letters, as already mentioned, would form only two peculiarities in his language, easily followed by his friends.

The numbers may be as easily taught as any other adjectives, and on exactly the same principles. Each mark is, indeed, significant, which is not usually the case with the letters of which words consist. But this circumstance cannot produce any embarrassment. When he knows the meaning of the word *cup*, that of 2 *cups* and 3 *cups* is easily conveyed; and the variation of the examples will soon suggest the generality which is to be attached to the numerical sign. The meaning of the units and their combinations may be taught by means of small bodies, such as pins, marbles, or pebbles. The regularity of the decimal numeration, and the power of expression obtained by the combination of the ciphers, are not unlikely to be contemplated by him with considerable relish. Information in these particulars may at first be conveyed to him by means of pins stuck in a flat cushion, or pegs stuck in holes in a board, in such an order as the following:

1	·
2	· ·
3	· · ·
&c.	
10	· · · · · · · · · ·
11	· · · · · · · · · · ·
&c.	
20	· · · · · · · · · · · · · · · · · · · ·
&c.	
100	· ·

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The extent of arithmetical instruction which it will be proper to give him must depend on the degree of zeal and intelligence which he displays in the progress of his lessons. Limited as the objects are which solicit his curiosity, he may discover in arithmetic, geometry, or some other science, a degree of application, and acquire a degree of proficiency, which to the greater part of men would appear miraculous. This would undoubtedly be highly interesting; but it is evident that no particular anticipations on the subject can be cherished.

That he may be enabled to trace minutely the lapse of time, he should have access to examine the dial-plate of a clock with the hours marked with common ciphers in relief; or a watch should be given to him with a dial-plate formed in that manner. This piece of instruction may easily be conveyed independently of any other, and the exercise is likely to improve the regularity of his habits.

If these and a variety of similar expedients are attended with the desired success, trials will naturally be made to reduce, for the sake of convenience, the size of all his characters. When they are near the minimum, he will now be able to make the circumstance distinctly known. As soon as the most eligible size is discovered, a simple printing-press may be provided for him, which will serve the same purpose which pen and ink do to others, and his friends may use it for furnishing him with pages of interesting anecdotes or instructive lessons, to occupy occasionally his moments of solitude.

In his communications with others, it will not long be necessary for him to employ tangible letters. He will find, that with certain materials he can write words which, though not palpable to himself, are understood by others. By making this

 discovery,

discovery, he will learn somewhat more particularly the nature of the advantages which the sense of sight confers. This is to a blind man an interesting article in the natural history of his species.

If he could be brought thus far, much additional improvement would soon follow. If, on the contrary, his want of application, or habits already too deeply fixed, should render him incapable of learning so much, it is evident that a degree of communication considerably short of this would be to him a most valuable acquisition. The probability is, that, after a little time, he will take the same delight in prosecuting his education which a child does in learning to understand what is said, and to make use of language.

It is by giving an impulse to his ardour, and thus guiding his will, that any person will be able to give to his mind, which is apparently desultory, a regular direction to a valuable object. His present unwillingness to submit to the dictates of other persons is a feature of mind which he possesses in common with a great proportion of mankind. Many individuals who had disdained the discipline of a school in early life, and appeared inferior to all their cotemporaries, have afterwards, to the great surprise of those whose views of human nature were superficial, acquired the highest proficiency in a branch of knowledge which had accidentally attracted their interest, or have begun to prosecute literature and science in an order suggested by their own thoughts, and rendered themselves illustrious as universal scholars. Some of these singular features of character, as well as that degree of indifference which is much more common, have been produced by the injudiciousness and formality of the prevalent methods of education. It is by the application of a more considerate policy to the developement of the human powers that we are to expect to communicate most effectually

fectually to persons of various natural faculties and various dispositions the benefits of education. Principles analogous to those which have been recently introduced into schools will afford great assistance in the education of JAMES MITCHELL; and the study of such an interesting and difficult case as his, together with the various incidents which must arise in the execution of a carefully concerted plan, may throw some light on that sort of management which is required both for domestic tuition and for the universal interests of education.

Miss MITCHELL's letter to Mr PARKER *, giving an account of the failure of her efforts, shews the necessity of method as well as of delicacy in the undertaking. A plan which imposes less labour on him, and leaves more to his own activity as excited by powerful motives, will be so far in less danger of producing disgust. Although his present local situation were not altered, I should entertain some hopes of success from the plan which I have described, in the hands of Miss MITCHELL. But, if these should be disappointed, the object might with certainty be attained by a change of measures not difficult of execution. Miss MITCHELL allows that another person, possessing more controul over her brother's mind, might succeed better than she did, and thus shows that she did not suffer the temporary disappointment to produce irrational discouragement. Before reading this letter, we might naturally enough consider her presence as necessary to his comfort and improvement. But it now appears evident that the plan of instructing him in language would be commenced with greater advantage by a temporary separation, provided he were connected with an establishment otherwise suitable. He should be under the care of a stranger, who has a perfect understanding of the mode of conducting his lessons, and who would carry it into effect by an authority from which there would lie no ap-peal.

* See p. 130. of this Volume.

peal. In such a situation, he might also be favourably affected by the novelty of finding every surrounding object accompanied by a label, of which he would be provided with a duplicate. The discipline employed must be steady; but there is no occasion for harshness. Although he has hitherto been suffered to follow the impulse of his own wishes, it will be remembered that in one instance a mild and decisive step was taken to deter him from exposing himself to danger *. He was observed creeping over a narrow wooden bridge which crossed a river at a point where the stream was deep and rapid; and, that he might not again make the same hazardous attempt, a servant was directed to plunge him, as soon as he was secured, once or twice into the river. This expedient had the desired effect. All the discipline required would partake of this character, and would only differ in being dictated by a regard for his improvement, and not by any apprehension for the safety of his life. The disappointment of his habitual expectations of indulgence, and the passion or chagrin which this would at first occasion, should not be considered as any greater obstacles to perseverance than in proceeding with the domestic controul of children. Miss Mitchell's influence over him is highly reputable to her, as showing her dexterity in kindness under circumstances of difficulty; but it would be an object of some importance to make him easily manageable by other persons. There is every reason to think that, by a very short separation and subjection to well-concerted rules, he would, on returning to the society of his friends, find himself much happier than before, and capable of giving them more pleasure, and that it would afford him great delight to find that his sister understood the language which he had learned, and could maintain with him intelligent communications. Finding himself now of greater importance in the scale of society, and possessing

* See Vol. vii. p. 27.

sessing a more accurate knowledge of the occupations and habits of others, he would probably acquire practical patience, and accommodate himself more completely to the convenience of those around him.

One of the most promising advantages likely to be conferred on him by the acquisition of language consists in the opportunity afforded to his friends to make known to him the utility of an operation for the cataract. It would now be indispensable to obtain his consent before proceeding to any operation. The employment of force would be equally unjustifiable as in the case of any other adult. If he is reluctant, the operation ought to be delayed, and, in the mean time, easy experiments employed, and advantage taken of accidental occurrences, to impress on him the wisdom of submitting to temporary pain for the sake of important subsequent advantages.

To some it might appear probable that this young man, from beginning to learn language comparatively late in life, would be the better enabled to gratify philosophical inquirers with a comparison between the state of his ideas before and after making that acquisition. On this subject, however, they ought not to entertain very sanguine hopes. Our past impressions, especially in their defective points, soon cease to be distinctly traced. The only exceptions to this general fact are those crude ideas of an inexperienced age which give amusement on recollection by their grotesque and incongruous character. The most prominent revolutions of his mind would probably affect the ideas which he entertains of the habits and sentiments of other persons. A statement of his views as in this respect improved, though it might be considered as little fitted to elucidate the more recondite doctrines of pneumatology, could not fail to prove interesting to all who take pleasure in the study of human nature.

APPENDIX.

APPENDIX.

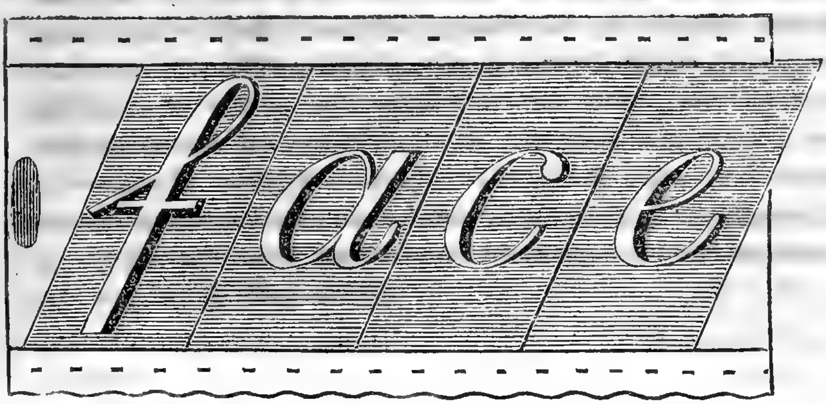
17th June 1816.

It is only within this fortnight that any preparations have been made for the prosecution of the plan described in the preceding paper, and they would not have been mentioned in their present imperfect state, were it not that this is the last meeting of the Society for the season. All the experiments made have consisted in attempts to find out the most eligible method of providing tangible words, a point which must be fixed upon before the plan is communicated to those on whose care the execution of it is ultimately to devolve.

Moulds of wood and of lead have been tried, and a specimen of a word formed in plaster of Paris from a set of leaden moulds is here shown to the Society. After various trials, the size of letter fixed upon is nearly that exhibited in the wooden cut accompanying these observations. It is found that a greater degree of distinctness to the sense of touch is gained by placing the strokes at a considerable distance from one another, than by increasing the perpendicular depth of the letters. A character similar to what is called a *round hand* in writing, though not easily made elegant on a large scale, is therefore the best adapted to the present purpose. It is evident from the specimen now produced, that plaster of Paris is objectionable for its brittleness and weight, as well as the expence and trouble connected with it. Some of these objections lie also against wax, clay, and paste. Letters formed of paper or thin pasteboard, on a plan similar to that of Haüy already mentioned *, will be far more suitable, and, from two specimens now laid before the Society, it will be seen that they are sufficiently palpable, sufficiently strong, and also very economical.

* Page 145.

nomical. Both were formed from hollow moulds. The pasteboard was first moistened, then laid on the mould. In making the one, a layer of fine sand was placed above it, and over that the weight intended to produce the impression. In making the other, the thin pasteboard was carefully forced into the hollow lines of the mould with a paper-folder; small quantities of moist paper were then stuffed into the cavity to fill it, and over this the weight was laid till it was dried. A subsequent improvement has occurred, for which I am indebted to the ingenuity of Mr John Ruthven (the inventor of the *Ruthven printing-press*), who is now executing in brass a set of types adapted to this object. This consists in forming hollow moulds, of the size and shape of letter now fixed upon, and also letters in relief corresponding to them, by which the moistened paper or pasteboard may be forced into the moulds. This method produces an impression in a most correct and speedy manner, and any word in one entire piece may be conveniently formed. The nature of these materials is such as to enable any person, when in possession of separate letters, to fix them together extemporaneously, so as to form words which can either be kept in that state or taken to pieces at pleasure. The separate letters may be introduced in their proper order into a running case formed of paper or pasteboard, and the word will then have this appearance.



 This

This figure represents a slip of pasteboard, which has two narrow slips stitched to it, the one along its superior, and the other along its inferior margin, so as to admit the letters to be introduced under their edges, and held in their places. The hole at the beginning of the word serves for fastening a loop by which it may be suspended; it also serves as a mark for the beginning of the word, and thus directs the pupil in the method of holding it correctly. A page of pasteboard, consisting of a succession of lines formed in this manner, will be a convenient surface for containing tangible sentences, instead of the surface of wax formerly mentioned *. The letters are here placed at a considerable distance, which will be proper in the first instance, that their forms may be separately traced. It will afterwards be easy to cut off the superfluous paper from each, to bring them nearer together, and thus save space †.

It is one great advantage of the preceding plan that it may be executed at very little expence. If it succeeds in his present situation, the expence will be a mere trifle. If not, a situation in the country will still be the best adapted to him. Any intelligent person possessed of sufficient leisure, (a country clergyman for example,) might, by having him for a certain time an inmate of his house, execute the whole with little difficulty. All the expence would consist in a suitable board for twelve, or perhaps only for six months. For providing this, there certainly will be no necessity for having recourse to any steps which the manners and feelings of our country

* Page 145.

† This plan is evidently adapted to the use of the blind in general; and experiments may be made with great facility on that class of persons, whose claims on the attentions of society are fully recognised, though they are highly privileged when compared with the subject of these observations. Such experiments, it is to be hoped, will be speedily attempted, if they have not hitherto been made.

country render exceptionable. There are undoubtedly individuals ready to expend much more in philosophical experiments of a less interesting kind, who, if confident of success, would be happy to have an opportunity of offering, in this instance, a tribute to science. I hope that some experiments will soon be made by Miss MITCHELL, on the plan now described, assisted by such observations as occur to different gentlemen who are friendly to the undertaking. If these should fail, it is still to be hoped that the object will not be lost for want of further efforts.

The utility of it is not limited to one individual. It has been justly remarked that other parallel cases have very probably occurred, which, from a false delicacy, have been concealed from general notice. Accounts have lately been received of one now existing in the United States of America. It will be highly pleasing to find that two individuals so far removed from the rest of mankind in their present means of communication can be taught the same language, and enabled to compare together the ideas which they have acquired relative to themselves, to the world in which they dwell, and to the rest of mankind, who must appear to them intelligences of a superior order.

We feel sensations allied to compassion when we contemplate their situation. Part of this, however, arises from the influence of a comparison with our own advantages, which we are too apt to consider rather as necessary to our being than as contributing to our enjoyment. The scale of happiness is not regulated by the degree of these advantages; and, instead of deteriorating the hearts of these individuals by officiously teaching them to mourn over their misfortunes, we shall be better employed in collecting for our own use materials towards the cultivation of the art of happiness, by observing that

serene contentment which, when not corrupted by other persons, both the blind and the deaf so often exhibit. When the blind are addicted to complaints on this subject, it entirely arises from the superfluous lamentations which they hear from those around them. This occasionally exists in circles in which a sickly and unprofitable sensibility is cherished; but it chiefly abounds among persons who are indigent and neglected, and who indulge in the habit of bewailing their fate as a part of the talent of the mendicant. In these particulars no deficiency of manliness seems to have been betrayed by James Mitchell, though his privations are doubled; and, as he advances in his intercourse with others, he is not likely to receive his impressions in a school in which any feelings tending to generate depression will be fostered. The glimmerings which he receives of his own state by a comparison with the advantages of others will add to his stock of information, and will contribute to render him more reasonable, without producing any mortification of his feelings. He must find himself always dependent on others; but in this there is nothing to render the mind abject. Every man is dependent on the arrangements of nature and of society; and that species of erectness and fancied independence which arises from a forgetfulness of the condition of man is rather to be condemned as a poor and ignorant pride, than regarded as a valuable prerogative accompanying the possession of external advantages.

VIII.

VIII. *On the Optical Properties of Muriate of Soda. Fluate of Lime, and the Diamond, as exhibited in their action upon Polarised Light.* By DAVID BREWSTER, LL. D. F. R. S. LOND. & EDIN. & F. A. S. EDIN.

[*Read Nov.* 20. 1815.]

IT has long ago been remarked by the Abbé HAUY, that the property of Double Refraction is not possessed by any of those crystals the form of whose integrant molecule is distinguished by its symmetry, such as the Cube and the Regular Tetraedron. This class of minerals comprehends *Fluate of Lime*, *Muriate of Soda*, *Spinelle Ruby*, *Muriate of Ammonia*, *Alum*, and the *Diamond;* and though M. HAUY had examined only a very small number of doubly refracting crystals, yet, with the exception of the *Diamond*, which I have found in many cases to possess the property of Double Refraction, his remark was confirmed by the experiments of MALUS, BIOT and myself, who considered all the crystals of this class as exercising no more action upon polarised light than a mass of water.

No explanation was offered of this singular anomaly, till M. BIOT discovered that all doubly refracting crystals are divisible

divisible into two classes; one of which is represented by calcareous-spar, and the other by rock-crystal. M. De La Place * had already shewn, in his fine theory of Double Refraction, that all the phenomena of calcareous-spar could be explained by supposing the deviation of the extraordinary ray to be produced by a repulsive force, directed from the axis, and proportional to the square of the sine of the angle which the extraordinarily refracted ray forms with the axis of the crystal. In like manner, M. Biot perceived that the phenomena of double refraction in rock crystal were explicable by an attractive force directed to the axis of the crystal, and following the same law; and he was led to suppose, that *muriate of soda*, and *fluate of lime*, &c. composed an intermediate class of crystals, in which there was neither an attractive nor a repulsive force, and consequently neither a division nor a polarisation of the transmitted pencil.

In this state of the subject, Philosophers will no doubt be surprised to learn, that *muriate of soda*, *fluate of lime*, *the Diamond*, *Alum*, and probably all other crystals of the same class, have actually the property of Double Refraction, but under circumstances of such a singular kind, as to entitle them to be regarded as a new class of doubly refracting crystals.

The specimens of fluor-spar in which I first recognised this property, are represented in Plate IV. Figs. 1, and 2. and had the following dimensions:

Fig. 1.	Fig. 2.
AB = 1.03 inches,	AB = 0.80
AC = 0.6	AC = 0.5
AD = 0.6	AD = 1.0

Both

* *Sur la loi de la Refraction extraordinaire dans les cristaux diaphanes.* Mém. de l'Institut, 1809.

Both of these crystals seemed to inclose a number of cubes of different tints, having their faces parallel to the external cube.

When polarised light was transmitted through the faces ADC, BEG, it was distinctly depolarised, the neutral axes AD, AC being coincident with the sides, and the depolarising axes AE, DC with the diagonals of the square faces; and, what was still more remarkable, in all the specimens there were portions of the crystal where the polarised light suffered no change. In these experiments, the tint polarised by the spar was a *blue* of the first order, having a *pale red* for its complementary colour.

In order to examine the tints with more correctness, I combined the cube of fluor-spar with a plate of sulphate of lime, which polarised a *brilliant blue* of the second order, having an *orange-yellow* for its opposite colour. The blue was changed into a *scarlet-red*, and sometimes into a *purple-red*, and the complementary *orange-yellow* into a *yellowish-white*. When the cube was turned 90° round, the *blue* was changed into a pale *yellow-green*, and the complementary *orange-yellow* into a *yellowish-purple*.

This change of colour was consonant to the laws which regulate the action of all crystals upon light; but I was surprised to observe, that when the cube of fluor-spar remained stationary, there was one portion of it at *m*, Fig. 3. which made the *blue* colour *red*, and the *orange-yellow* a *yellowish-white*, while another portion, at *n*, made the *blue* colour *green*, and the *orange-yellow*, *purple*. In another specimen, I found the same opposition in the effects produced by two different portions, *m*, *o*, which were separated by a third portion that had no action upon light, the part *o* producing the same effect that *m* would have done when turned 90° round, and *m* the same effect

effect as *o* when turned 90° round. The preceding phenomena were exhibited in every specimen that had a considerable thickness.

My experiments on *muriate of soda* were made with large masses of various sizes, from half an inch to three inches in length. They all exhibited the same properties as fluor-spar, the depolarising axes being coincident with the diagonals of the square faces, and the neutral axes with their sides. In the largest pieces, the polarised tint was a fine *blue*, with a *pale yellow* for its complementary colour, and the oppositely polarised portions produced by different parts of the mass, were arranged in streaks parallel to one of the diagonals AC of the cubical face ABCD, as represented in Fig. 4. Similar phenomena were exhibited in large pieces of transparent *alum*.

In my first experiments on the *Diamond* *, the specimens which I employed had very uneven surfaces; but I have lately repeated them with nine flat diamonds, for which I was indebted to John Robison, Esq. Almost all these specimens depolarised the light in separate spots, of an irregular shape, and the depolarising portions had opposite structures, like the specimens of muriate of soda and fluor-spar which have already been described. The appearance of these diamonds, when exposed to polarised light, is represented in Fig. 5. One of them, however, had a more perfect crystallisation, and exhibited four parallel fringes, as shewn in Fig. 6. The tints were a white of the first order. When the interior fringes of a plate of crystallised glass were held parallel to the fringes *a d*, *c f*, the difference of their effects was produced,

* See *Phil. Trans.* 1815, p. 29.

duced, but when the same fringes were held parallel to the fringe *b e*, the sum of their effects was produced. Hence it follows, that the structure which produces the fringes *a d*, *c f*, is the same as that of one class of doubly refracting crystals, and the structure which produces the fringe *b e*, the same as that of the other class. An effect exactly similar to what is shewn in Fig. 5. is exhibited by the sclerotic coat in the eyes of fishes, and may be produced by crushing a piece of soft isinglass, or by pressing a mixture of rosin and bees-wax *, between two plates of glass.

The preceding experiments entitle us to conclude, that *muriate of soda*, *fluor-spar* and *the diamond*, combine in the same specimen three different structures, and form a new class of doubly refracting crystals. In some parts they act upon light like that class of crystals in which the deviation of the extraordinary ray is supposed to be produced by an attractive force. In other parts they act upon light like the other class of crystals in which the extraordinary ray deviates from the axis in virtue of a repulsive force; and in intermediate portions they exhibit that mean structure, in which the light is urged neither by attraction nor repulsion, and where there is neither a polarisation nor a division of the transmitted pencil. If the laws which regulate the crystallisation of these minerals had been allowed an undisturbed operation, it is probable that the crystals would have had a perfect cube or an octahedron for their primitive form, and would have exhibited none of the phenomena of double refraction.

The slightest irregularity, however, in the operation of these laws, would produce a deviation from the perfect primitive form, and the crystal would therefore deviate from the intermediate class into the attractive and repulsive classes, and would

* See *Phil. Trans.* 1815, p. 32, 33.; and 1816, p. 172, 173.

would thus acquire the three different structures which we have actually found it to possess.

If this view of the subject be correct, it will follow, that the cubical and octahedral forms are intermediate between those which belong to the attractive and repulsive classes of doubly refracting crystals;—that a deviation from these forms on one side will produce the structure of the attractive class, and a deviation on the other side the structure of the repulsive class; —that the force of double refraction increases with that deviation, and that there is a certain primitive structure belonging to each mineral, by which its class may be ascertained.

The imperfect state of crystallography does not enable us to determine what this structure is; but when this science shall have made farther progress, we may probably be able to ascertain from the crystalline forms of minerals, both the character and the intensity of their doubly refracting force.

APPENDIX.

Edinburgh, July 4. 1816.

M. Biot, who had an opportunity of examining the greater part of the preceding paper in MS. in October 1815, has stated in his *Traité de Physique*, tom. iv. p. 573, newly published, that the polarising structure which I have discovered in Muriate of Soda, Fluate of Lime, and the Diamond, appears to be owing either to the effects of heat, or rapid evaporation. " C'est je crois à cela," says this eminent mathematician, " qu'il faut attribuer les indices de polarisation observées acci- " dentellement par M. Brewster dans certains echantillons de " cristaux

"cristaux qui par leur nature ne possedent point la double re-
"fraction. Enfin ce qui achéve de confirmer cette manière
"de voir, M. Brewster a encore imprimé les mêmes proprie-
"tés à des plaques de gelée animale, en exerçant sur elles ex-
"terieurement une pression passagere; de sorte que les cou-
"leurs paraissent tant que la pression dure, varient avec elle,
"et evanouissent quand elle cessé."

Had M. Biot repeated the experiments to which he alludes in the preceding passage, and compared the results with those produced by heat and rapid evaporation, he would have instantly seen that the two classes of phenomena are essentially distinct, and could not possibly have the same origin. In the polarising structure produced by heat, by rapid cooling, and by evaporation, the axes are constantly related to the edges, the angles and the surfaces of the bodies which are employed, and the nature and form of the optical figure which they exhibit, depend solely on the outline and on the thickness of the mass. In cubical and octahedral crystals, on the contrary, the polarising axes are related to the axes of the crystals themselves, and have no connection whatever with the shape or outline of the mineral. If we take a cube of muriate of soda, for example, which has not sufficient thickness for developing its structure, and expose it either to heat or pressure, we shall find it impossible to apply either of these powers in such a manner as to produce a crystallisation that has the smallest resemblance to the effect shewn in Plate IV. fig. 4. If the muriate of soda is thick enough to render its structure visible, then the crystallisation superinduced by heat or pressure may be seen at the same time with its own natural crystallization. The results obtained by means of the diamond, as shewn in fig. 6. and with which M. Biot was not acquainted when he wrote the preceding pas-

 sage,

sage, shew still more satisfactorily the difference between the two classes of phenomena.

I do not understand what M. Biot means by saying, that these discoveries were made by accident, (" observées *accidentel-* " *lement* dans *certains* echantillons"); for all the results in the preceding paper were obtained in the course of a regular train of experiments, undertaken for the express purpose of examining this anomalous class of crystallised bodies; and the polarising structure was detected, not in some specimens, but in every specimen of a proper thickness which I had an opportunity of trying.

In my experiments on Animal Jellies, I had repeated opportunities of observing, that a polarising structure, invisible in a single mass, could be rendered perceptible by an augmentation of thickness; and I was naturally led to suspect, that a similar result would be obtained by using large masses of muriate of soda, and fluor-spar, although, like M. Malus and M. Biot, I had sought for it in vain in ordinary specimens.

IX.

PLATE IV

Engraved for the Royal Society Trans. Vol.VIII. Page 164

Fig. 1.

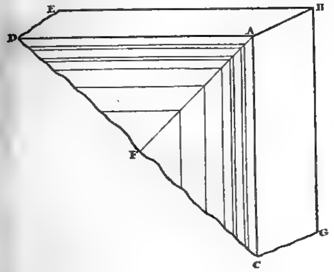


Fig. 2.

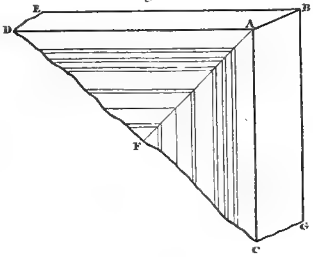


Fig. 3.

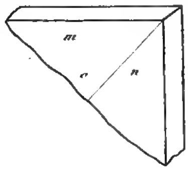


Fig. 4.

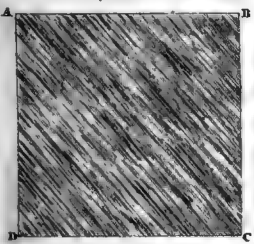


Fig. 5.

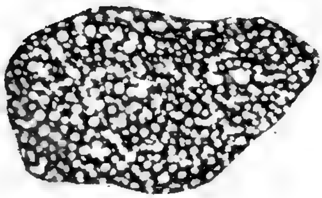


Fig. 6.

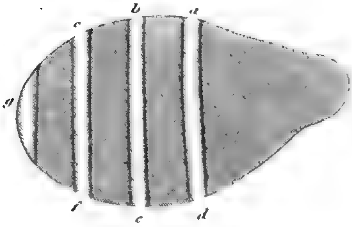


Fig. 7.

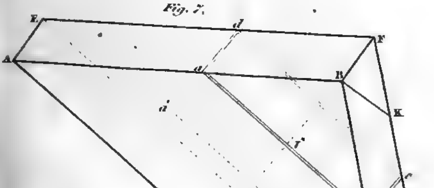
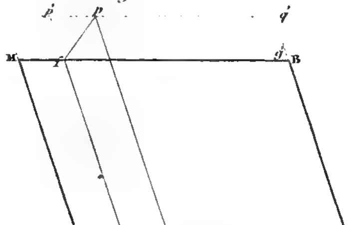


Fig. 8.



Engraved by W. & D. Lizars Edin.

IX. *On a New Optical and Mineralogical Property of Calcareous Spar.* *By* David Brewster, LL. D. F. R. S. Lond. & Edin. F. A. S. Edin.

[*Read April* 30. 1816.]

IT has been long known to those who have studied the optical properties of Calcareous Spar, that there are numerous specimens of that mineral which form a multiplicity of images, affected with the most brilliant colours. These colours were considered by Malus, and other philosophers, as the colours of thin plates, and were supposed to be produced by a film of air, included in accidental fissures or fractures within the crystal. I have already shewn*, from numerous experiments, that this opinion is erroneous, and that the various colours with which the images are affected, arise from the transmission of polarised light, through an extremely thin film or stratum of calcareous spar, which has different thicknesses in different specimens, but which has an invariable position, being always parallel to a plane passing through the longer diagonals of the rhomboidal faces.

Since that paper was written I have obtained some very perfect specimens of calcareous spar, which have enabled me to obtain several new and interesting results. One of these specimens

* See *Phil. Trans.* 1815, p. 270.

mens is represented in Plate IV. Fig. 7. where ABC is the greater angle of the rhomboidal face, and *a b c d* the interrupting stratum or film of calcareous-spar. Now, if ABCD were a common specimen of calcareous-spar, a ray of light incident upon either of the faces *a*BF*d* and *b*BF*c*, and transmitted through the face ADHE, would be separated into two pencils; and if these two pencils were received upon another prism of spar, four pencils would be formed, and two of them would vanish at every quarter of a revolution of the second prism. In the preceding specimen, however, when a ray of light is incident upon *a*BF*d*, or *b*BF*c*, and transmitted through ADHE, it is divided into two pencils; but these pencils have suffered such a change in passing through the stratum *a b c d*, that when they are received upon a second prism of common calcareous-spar, *none of them will vanish in any position of the second prism, but will continue visible during the whole of its revolution.* But if the ray of light is first incident on the surface ADHE, and emerges from *a*BF*d*, or *b*BF*c*, it possesses the same properties as if it had passed through the common specimens of calcareous spar.

The phenomena now described, are exhibited in every specimen of calcareous spar with an interrupting stratum, and cut in the manner shewn in Fig. 7.; and though they appear at first sight very perplexing, yet they are capable of the most satisfactory explanation. They are visible only when the interrupting stratum *a b c d* intervenes between a prism *ab*Bc*F*d and a flat plate ADH*bcd*E; for when the stratum is interposed between two prisms, the multiplication of images takes place as described in the paper already referred to.

The interrupting stratum *a b c d*, is crystallised in a different manner from the rest of the rhomboid; that is, its axes are not placed symmetrically with those of the mass which incloses

closes it; and from this circumstance arise all the phenomena we have mentioned. When a ray enters the surface BF*da*, or BF*cb*, it is divided into two pencils, polarised in opposite planes; and when these two pencils reach the interrupting stratum *a b c d*, they are depolarised by that stratum, in the same manner as if they had been acted upon by a film of sulphate of lime, and each pencil consists of two pencils, oppositely polarised, but not separated from each other. These two compound pencils have their polarisation again changed, by the flat plate AD*bcd*E; but as it cannot separate the oppositely polarised portions, the two pencils emerge from the face ADHE in such a confused state, that none of the four pencils into which they are subdivided by the application of a second prism, can vanish in any part of its revolution. Hence, the reason is obvious, why the pencils vanish when the ray of light is first incident upon the face ADHE; for in this case, the compound pencils, when transmitted through the stratum *a b c d*, have their opposite polarised portions arranged into two pencils, polarised in an opposite manner, by the action of the prism B*abc*F*d*.

Let us now endeavour to determine what is the position of the axes of the film relative to those of the surrounding mass. Since the film *a b c d* has the power of completely depolarising the incident light, we are entitled to infer, that its depolarising axes are coincident with the neutral axes of the rhomboid; or, what is the same thing, that the axes of the one are inclined 45° to the axes of the other; and hence it will follow, that the film *a b c d* has the same properties as if it had been detached from the place *a′ b′ b a* on the upper surface ABCD, and had slid down into the position *a b c d*. This inference is fortunately capable of the most rigid demonstration. Let MBFN, Fig. 8. be the principal section of the rhomboid; or a section of Fig. 7. on the line FB*f*, and let *f g q p* be a magnified section of the interrupting

interrupting stratum or film $a\,b\,c\,d$, Fig. 7. The upper and lower edges, $f\,p$, $g\,q$, of the stratum, are so distinctly crystallised, that, by the aid of my reflecting goniometer, their inclination to MB, NF, or the angle Mfp, was found to be 141° 44′. Now, if we suppose that the film $f\,g\,q\,p$ had originally the position of $f\,g'\,q'\,p'$ on the upper surface MB of the rhomboid, and was brought into its present position by turning round f, it is obvious, that the angle $p'\,f\,p$ must be equal to the acute angle Bfg or BMN of the principal section. But this angle, or its equal Mfp', is known to be 70° 51′ 46″, as computed from the accurate measures taken by Malus: hence the angle $p'\,f\,p$ is also 70° 51′ 46″; and the whole angle Mfp is equal to double of either of these angles, or 141° 43′ 32″,—a result which coincides almost exactly with the angle which we obtained from direct experiment.

Here, then, we have a new fact in Mineralogy, of which I believe there is no other example, viz. That the crystallisation of a vein is regulated by that of the mass which contains it, the axes of the vein being constantly inclined 45° to those of the rhomboid. Nor is this a fact of rare occurrence. The specimens of calcareous spar which are intersected with these veins, are as numerous as those which are free from them; and those which occur in the trap rocks on the east coast of Scotland, near Montrose, invariably possess this property to such a degree, that they are rendered semi-transparent by the numerous veins with which they are traversed. When a candle or a luminous object is viewed through this kind of spar, it is multiplied in such a manner, that the images are heaped together in the most regular forms, and are tinged with the finest colours, constantly varying with the inclination of the specimen.

When these specimens are carefully examined, they exhibit what has never before been observed, two or more sets of veins

veins or interrupting strata, one of which, like *a b c d*, Fig. 7. is parallel to BF, while the other sets are parallel to BC or BA, the common sections of the three surfaces which contain the solid angle. When the light is incident almost perpendicularly upon one of these specimens, so as to pass through two of the veins, *nine* images are formed; and there is a particular position of the incident ray, when all these images vanish, except the colourless image in the centre. This effect can be imitated by the transverse combination of two specimens that have only one vein or interrupting stratum.

There is another phenomenon exhibited by this interposed film, which throws new light on the cohesion of solid bodies. The two surfaces of the film, though in *physical* contact with the surrounding mass, so as to adhere to it with the greatest force, are nevertheless not in *optical* contact with it. There is a distinct reflection of light at the touching surfaces, and the distance of these surfaces is demonstrably greater than $\frac{50}{1,000,000}$ or $\frac{1}{20,000}$ of an inch *.

Now, since the surfaces of the interposed film are in every case kept at such a distance from those of the adjacent mass, and cannot be brought nearer to it by the force of screws, we are entitled to conclude, that the particles of calcareous-spar are not capable of coming into perfect contact, unless when they are arranged symmetrically; that is, when the axes of all the elementary rhomboids are parallel. When these axes are inclined to each other, the attractive forces by which the particles are held together, are so much weakened, that they are incapable of forming a mass perfectly continuous, and devoid of all internal reflections. This fact is the only unequivocal indication with which we are acquainted, of the existence of a polarity in the particles of crystallised bodies.

* See *Phil. Trans.* 1816, p. 72, 73. where some analogous experiments are described.

[illegible]

X. *On the Ancient Geography of Central and Eastern Asia, with Illustrations derived from Recent Discoveries in the North of India.* *By* Hugh Murray, Esq. F. R. S. Edin.

(Read April 29. 1816.)

THE descriptions which historians and geographers have transmitted to us of the ancient world, are not generally deficient either in copiousness or accuracy. The theatre of those great events, which still interest mankind, may be commonly ascertained with sufficient precision. The distinct knowledge of the ancients, however, was limited to a certain sphere; after passing which, clouds always begin to envelope it. The almost total change of names, the uncertainty as to their itinerary measures, and the defects of their mathematical geography, leave no perfectly fixed point on which we can rest. Hence, even where copious and interesting details are given, it is often difficult to determine to what region, or to what nation, these descriptions refer. The question may appear sometimes to be one of curiosity only; yet the curiosity seems natural and liberal, and it is often connected with interesting questions relative to the history of the human species. It illustrates the extent to which commercial enterprise and activity had reached in ancient times; and often, by enabling us to compare the modern with the ancient aspect of the same re-

gion,

gion, it qualifies us to ascertain the progress which mankind have made, during so long a succession of ages.

There are perhaps no regions with regard to which this question possesses equal interest, as those which formed the eastern extremity of the ancient world. The faint and decaying light of science there shone, not upon inhospitable deserts, not upon the abodes of rude and pastoral tribes, but upon civilized, populous, and commercial regions, which, in all the arts and improvements of life, were not perhaps much inferior to the Roman empire during its most flourishing era. Yet the question, what regions these were, is involved in as deep obscurity as any which has occupied the inquiries of the learned. Many interesting points are still totally unfixed, and afford room for the most discordant opinions. Having been induced to study this subject with peculiar attention, it has appeared to me, that some light might still be thrown upon it, by a careful analysis of the ancient statements, as well as by attentively comparing them with some discoveries which have recently been made in that quarter of the world.

The two nations whose territory formed, to the ancients, the eastern extremity of the known world, were the *Seres* and the *Sinæ*, of whom the former were approached by land, and the latter by sea. Of these the Seres were the most celebrated and interesting people, and will form the main object of the present inquiry ; but it may be convenient to begin with fixing the position of the Sinæ. The approach to their coast is thus described by PTOLEMY. After passing the mouth of the Ganges, and a long extent of coast beyond, navigators rounded a large peninsula, called the Golden Chersonese. Then passing a great bay (Magnus Sinus) they came to a coast, which was that of the Sinæ, which extended from north to south, with an ocean on the west. The early modern opinion was, that the Sinæ

Sinæ were, either in whole or in part, the inhabitants of Modern China. But D'ANVILLE, who largely reduced the world of the ancients, fixed the Magnus Sinus in the Gulf of Siam, and allowed only a limited navigation along the coast of Cambodia. M. GOSSELIN, with bolder scepticism, fixes the Sinæ on the coast of Siam, and never allows the ancients to have passed the Straits of Malacca.

In comparing these three statements, there cannot, I apprehend, be the smallest hesitation in preferring the one last mentioned. There positively is, beyond India, no coast, besides that of Siam, which has an ocean on the west. PTOLEMY mentions no island of a magnitude which could at all correspond to that of Sumatra. Even the Golden Chersonese, though it may suggest at first sight the peninsula of Malacca, will, when its details are examined, be found better to correspond to that of Ava and Pegu. This solution having been acceded to by Mr PINKERTON, by Dr VINCENT, and by all the eminent geographers of the present age, its correctness may probably be considered as a point finally decided.

We proceed, then, to the question respecting the Seres, a people who, by their mysterious remoteness, their wealth and civilization, and the peculiarities of their national character, excited an extraordinary interest in the ancient world. The information of the Greeks and Romans respecting their territory, as well as a long series of intervening regions, was chiefly derived from a great mercantile caravan, which, setting out from the Bosphorus, traversed Asia from west to east, till it arrived on the frontier of Serica. This communication does not appear to have been formed, till about the first century, during the most extended period of Roman power. Its object was to supply that empire with the luxury of silk, the use of which, from being a rare appendage of greatness, had become common to

almost

almost every class of society. The reports of the caravan merchants were collected and committed to writing by MARINUS of Tyre, whose compositions have perished; but the corrected substance of them is found in the great geographical work of PTOLEMY. The statements of PTOLEMY, therefore, combined with some supplementary information from PLINY and AMMIANUS, must form the authority on which this question is to be decided.

The earliest modern opinion which I find stated upon this subject, is, that Serica was Cambalu, or the kingdom of the Great Khan, that is, the original dominion of Zingis. China, then, was the Sinarum Regio. Before the time of D'ANVILLE, however, the prevalent sentiment came to be, that the northern part of China was the seat of the Seres, the southern that of the Sinæ. VOSSIUS goes farther, and declares that he who doubts if the ancient Seres be the modern Chinese, may doubt as reasonably if the sun that shone then be the sun that shines now. As that learned and acute writer, however, has not explained the ground on which so peremptory an opinion was formed, it has not met with the attention which perhaps it merited. D'ANVILLE was the first who applied to this question that careful and systematic analysis which forms the only true mode of solution. Having brought the Sinæ to Cambodia, he carried westward also the position of the Seres. He assigned to them an extensive region of eastern Tartary, reaching from the territory of the Eygurs, or Igours, to the north-western frontier of China, of which it included only the projecting corner of the province of Chensi. Mr PINKERTON goes still farther, and places Serica in Little Bucharia. But M. GOSSELIN, with his usual boldness, has struck out an entirely new path. He finds Serica in the north of India, in the district of Serinagur, including a portion of Thibet.

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The writers now mentioned, however widely discordant as to other particulars, seem to agree in one point, that of treating with contempt, and almost with ridicule, the ancient idea which extends Serica to China. Dr VINCENT alone, who thinks always for himself, has declared his adherence to the latter opinion. His subject, however, has led him to rest almost entirely on the maritime testimonies, which do not, I confess, appear to me so decisive as to the learned writer. They are contained in the narratives of ARRIAN and COSMAS INDICOPLEUSTES, persons who never passed Indostan, and collected only vague and inaccurate reports of the regions beyond. The testimony of such writers, it would appear, can never be put in competition with that collected from a company of merchants, who, if they did not enter Serica, at least habitually trafficked on its frontier. I certainly concur, therefore, with D'ANVILLE and the other geographers, in considering PTOLEMY as the main authority by whom the question must be decided. But, in adopting their premises, I have been led pretty confidently to a conclusion the opposite of theirs. The works of PTOLEMY and his cotemporaries appear to me to contain a series of statements which fix down, in a very decided manner, Serica as China. As results quite opposite have been drawn from every analysis yet made of these statements, and as they appear to me to involve a view of the entire geography of central Asia, widely different from any at present received, these circumstances, I hope, may plead my excuse for the unexpected length to which the discussion has extended.

Considering the decidedly opposite opinion which has been held by the most eminent geographers of the present age, I should perhaps have hesitated in laying before the Society the result of my inquiries, had they not been so strongly supported

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by recent discoveries in the north and north-west of India. These appear to have not only furnished new materials for the solution of the question, but to have laid open radical errors, which have hitherto darkened the view of modern inquirers. When these are removed, I am persuaded that the reports of the ancient writers will be found clear, consistent, and satisfactory, to a degree beyond what has yet been suspected.

The principle hitherto proceeded upon, by all modern geographers, is, that provided their system appears to be supported by a few names and particulars found in Ptolemy, they are at perfect liberty to impute to him any errors, however enormous, which may be necessary to fill up their hypothesis. M. Gosselin broadly lays down the maxim, that all precise knowledge, on the part of the ancients, terminated with the range of the Beloor; and D'Anville repeatedly warns his readers against expecting more than a very vague and general coincidence between the actual features of the country and Ptolemy's description of them. This last, he observes, must be corrected by the more copious and accurate information of modern times. My own researches, on the contrary, have led to a pretty decided conviction, that the ancients knew more respecting these regions, than has been, or is still known to the moderns; that they knew more consequently than those very eminent geographers who have treated their authority so lightly. I believe, if the statements of Ptolemy be taken simply as they stand, and be carefully collected and arranged, they will be found to exhibit correctly all the grand outlines of Central and Eastern Asia.

Considering the subject in this view, it may be advantageous, before entering upon the proposed analysis, to notice some preliminary facts, which may throw light on the general degree of knowledge possessed by the ancient writers respecting this

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part of the world. These are furnished by the very laudable efforts already alluded to, made by our countrymen in the East, to improve the geography of India, and the neighbouring regions, particularly by the recent mission to Caubul. A number of leading points have thus been satisfactorily settled; and the means are afforded of forming a comparative estimate between PTOLEMY'S information and that hitherto possessed by modern geographers.

One of the leading questions in Indian geography, has always been that relating to the course of the five great rivers that water the Punjab. It was ascertained by the gentlemen attached to the Caubul mission, that these, after forming two great branches, at length united into one, and poured their waters into the Indus by that common channel. They had uniformly been represented before as falling by two separate and somewhat distant channels. This is justly noticed by a learned writer in the Edinburgh Review, as one of the most important recent geographical discoveries. It certainly was such to the moderns: but it merely restored the delineation which had been given, nearly two thousand years before, by PTOLEMY. His map exhibits the five rivers, which, after forming two great branches, unite and fall into the Indus, precisely in the manner described by Mr ELPHINSTONE. PLINY'S testimony is to the same effect; he describes the Hydaspes falling into the Indus, *quatuor alios amnes afferentem.*

In endeavouring to prove the imperfection of PTOLEMY'S knowledge relative to the north of India, M. GOSSELIN pointedly refers to his placing the source of the Ganges in the Imaus (Himalaya) instead of deriving it from Thibet. Here also, however, PTOLEMY happens to be in the right. In 1808, the Supreme Government of Bengal, at the instance of the late Colonel COLEBROOKE, sent a mission to explore the origin of

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this celebrated stream. The party followed its course till it became little more than a rivulet. Their further progress was arrested by precipices, and by the snows, under which its infant course was buried. But they made such observations, and collected such intelligence, as left no room whatever to doubt that it descends *from* the Himalaya, and that the long course which our maps assign to it along the table land of Thibet, has no existence in fact. They found also, that, contrary to general opinion, the sources of the Jumna, and of the Sarayu, the head of the Gogra, were in the same chain, and at a small distance from that of the Ganges; as they had been placed by Ptolemy, who has not, indeed, been so happy as the moderns, or, indeed, as Pliny has been, in delineating some parts of their subsequent course.

The gentlemen employed in the Caubul mission, extending their researches to the north-west of India, have brought to light some other important features. One of the most prominent is the river Kaumah or Kama, which rising on the opposite side of the same range which gives origin to the Oxus, pursues a northerly course of nearly four hundred miles, till it falls into the Indus, of which it forms the largest western tributary. Near its junction it receives the river of Suaut, so called from a beautiful and fertile valley through which it winds. Of these streams, previous to the mission, no trace appeared in our modern maps; but in turning to Ptolemy, we shall find them both delineated, under the appellations of the Coe and the Suastes, with some variation as to magnitude, but correctly as to relative position and mode of junction; while the river of Caubul is noted as a branch from Parapomisus. It thus appears, that Ptolemy's knowledge of the northern boundary of India, though not perfect, was, on the whole, decidedly superior to that of the best informed modern geographers; and

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we find him delineating with success, grand geographical features, hid as it were in the most secret recesses of Asia, which had remained to them entirely unknown. Surely, then, it cannot be denied, that some regard is due to his authority, and that his delineations may possibly prove correct, even where they do not exactly coincide with those of our modern maps.

It is now time to proceed to the main object, of exhibiting PTOLEMY's delineation of Eastern and Central Asia, and comparing it with the actual features of that Continent. In this analysis, it will be convenient to follow the movements of the great caravan, from which he derived his information. Setting out from Byzantium, it proceeded through Assyria, Parthia, and other regions of Asia, to Bactria, a country still known under the appellation of Balk or Bulkh. It seems early to have been the *depot* for the caravan-trade of Central Asia, and had thus acquired a degree of wealth and splendour unknown to the barbarous regions that surrounded it. Thence the travellers proceeded into Sogdiana, which appears in Mr ELPHINSTONE's map under the modern appellation of Shoghnaun. The route, which had hitherto been easy and level, assumed now an entirely different aspect. Before ascending, however, into that vast mountain world, which Asia incloses in her bosom, it may be necessary to pause, and to take a brief view of its general structure.

It is well known that Indostan is bordered on the south by a table land of extraordinary elevation, called Great and Little Thibet. Two parallel chains, running from east to west, prop this mighty bulwark of Asia. The southern barrier is formed by that immense chain known under the names of Hindoo Coosh, and Hemalleh or Himalaya, which forms the northern limit of India. The whole extent of it is covered, to a great depth, with perpetual snow, and every measurement yet made,

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from Peshawer to Nepaul, has made it exceed 20,000 feet above the level of the plain, being higher than the highest peaks of the Andes. The whole is recognised by Ptolemy under the name of Imaus. The northern range, known by the uncouth appellations of Mooz Taugh or Karrakorrum, is, I apprehend, described by him under that of *Mons Ascataneas.* Though its absolute elevation appears to exceed the Himalaya, yet, from the high level of its base, it does not present so formidable an aspect. At the western extremity of these two chains, we find another, running at right angles to both, connecting them, and shutting in the western side of the table land. This is called the Beloor Taugh, (or Beloor, as *taugh* is merely the generic name of mountain.) It forms the eastern limit of Shoghnaun, the ancient Sogdiana, and thus coincides with the *Montes Comedorum*, the first ascent which the caravan is represented as having had to encounter. M. Gosselin, the most sceptical of modern inquirers, recognises the coincidence, so that, up to this point, there is no controversy.

Mr Elphinstone's map exhibits a route up the valley of the Oxus, and then ascending the Beloor, which has every appearance of being the route followed by the merchants upon this occasion. Having surmounted this laborious ascent, they descended into a plain, not arable or fertile, but affording abundant pasturage to the nomadic tribes by whom it was traversed. Then ascending a valley, by a route which I suspect to be that traced by Mr Elphinstone, up the valley of the Ladank, they came, first, to the Stone Tower, a singular appellation which is never explained, and then to another station, where a grand rendezvous took place of all the caravans that were proceeding to the great emporium of the East. This union was formed, with the view of overcoming an obstacle more formidable than any they had yet encountered,—the mighty

ty range of Imaus, which, after running for a great space from east to west, turned suddenly to the north, and stretching far into Scythia, separated that vast region into two portions, *Scythia within*, and *Scythia without*, *Imaus*. This chain forms thus the key to PTOLEMY's geography of Central Asia; and as my views respecting it differ very much from those hitherto entertained, it may be necessary to enter into a short discussion.

All modern geographers, so far as I know, have conceived that the Imaus must, in some shape or other, be identified with the Beloor. D'ANVILLE, RENNELL, GOSSELIN, PINKERTON, however they may differ in other respects, seem to consider this as a point placed beyond all dispute. Even Major RENNELL, while he clearly points out, that PTOLEMY's statements place the Imaus in a very different quarter, does not allow himself to suppose that those statements could be correct. Now I think it will appear on a very slight consideration, that every system which identifies the Imaus with the Beloor, is an entire subversion of all the descriptions and statements of PTOLEMY. Between these two ranges he places a great nation, or rather class of nations, under the appellation of the Sacæ. To their territory he assigns fifteen degrees of longitude; which, upon that parallel, and making every allowance for the defects of his graduation, cannot be less than from five to six hundred miles in direct length from east to west. This immense region, which equals half the breadth of Indostan, is reduced by the present system to what M. GOSSELIN calls one of the Gorges of the Beloor, a mere valley or glen between two of its branches. Thus, too, the Sacæ are entirely severed from India, of which however both PTOLEMY and STRABO describe them as forming the northern boundary.

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The delineation of the country of the Sacæ, will be found to correspond, in every respect, with that given by Mr Elphinstone of the Plain of Pamere and Little Thibet. It was bounded on the south by Indostan, from which it was separated by the ridge of Imaus. On the north it was bounded by the next parallel chain, *Mons Ascataneas*, which cannot possibly be any other than the Mooz Taugh, to whose name, indeed, it bears a rude resemblance. It extended eastward from the Montes Comedorum (the Beloor) to somewhat beyond the head of the Ganges; precisely the dimensions of Little Thibet. Again, it appears from Ptolemy, that the merchants, after ascending the Beloor, proceeded for several hundred miles to the south-east, which is precisely the direction of the table land. The mere aspect of Ptolemy's map, and that of Mr Elphinstone, present a striking similarity of form and geographical features. It seems plain, then, that geographers, in placing the Sacæ among the defiles of the Beloor, or extending them towards Cashgar, have disregarded the whole tenor of Ptolemy's statements. It is true, Edrisi has given us a region called Sakita, which seems merely to occupy the summit of the Beloor. But we must observe, that *Sacæ*, in Pliny and Ptolemy, was a generic name for a number of nomadic and pastoral tribes. It is well known that such names, among the civilized nations of antiquity, were usually formed by extending that of the nearest tribe to a long range of territory behind. It was natural, therefore, that the Sacæ, the first people who occurred, on ascending the mountains, should give name to a succession of similar districts, till another grand barrier of nature interposed. We are not, however, left here to mere inference or conjecture. Pliny, after mentioning the Scythian tribes east of Bactria, expressly says: "Persæ omnes illos Sacas appellavere, *a proxima gente*." The district of Sakita, therefore,

therefore, admitting it to be the country of the Sacæ proper, in no degree expresses the extent of that which, by PLINY and PTOLEMY, was called the Sacarum Regio.

There is one, and I think one only, important discrepancy. Neither the Ladauk, the Indus, nor, indeed, any river whatever, is to be found in PTOLEMY. This defect, though serious, cannot, I think, be placed against the perfect agreement of every other feature. My impression is, that PTOLEMY omitted these rivers from not knowing whither to lead them, for he, in common with the rest of the ancients, never suspected that the Indus or any river, penetrated across the snowy chain; and the last lesson which geographers ever learned, was that of confessing their own ignorance. The omission seems supplied by PLINY, who was not fettered by any such complete or mathematical delineation, and who expressly states, that several rivers, and, among others, *two* of great magnitude, traversed the country of the Sacæ.

But what is this mighty range of Imaus, or what other chain is there, besides the Beloor, which traverses Asia in such a direction? Whatever we may think as to where it really exists, there can be no doubt, as Major RENNELL indeed admits, where PTOLEMY meant to place it; for besides all the other indications, he states it as running northwards from Palibothra, and somewhat to the east of the sources of the Ganges. It ought then to be a chain dividing Great from Little Thibet, parallel to the Beloor, though at a great distance, and shutting in the eastern side of the table land. Such a chain seems scarcely recognised by modern geography; and yet there cannot, I apprehend, be the smallest doubt as to its existence, because it is now ascertained by the combined reports of Major TURNER, and of the Caubul mission, that in this quarter must lie the yet unexplored sources of the two greatest rivers of India,

dia, which here separate, and flow in opposite directions; on one side, the Indus, with its tributary the Ladauk; on the other, the Sanpoo, or Barrumpooter, both of which, at the highest point to which they have been traced, are already large streams, that have evidently flowed from a great distance. It is well known to those who are accustomed to such researches, that the point at which great rivers thus rise and separate, is always the most elevated of any region. The proof from analogy is therefore so strong, as to render perhaps any other argument superfluous. But if we collect the slender notices that exist, this feature will not be found wholly destitute, even of modern testimony. The little we know relative to Great Thibet, is chiefly derived from Duhalde's report of the mission of two Lamas from the court of China. Their object was, to make a map of that region, and to ascertain the sources of the Ganges. On their return they stated, that there *was* a great chain of mountains separating Great from Little Thibet; that from one side of this chain descended the Barrumpooter, and from the other, which they did not visit, the Ganges, with the Ladauk falling into it. It is now certain, that they were mistaken as to the Ganges; but I think it probable, that what they supposed to be that river, was really the Indus; because Mr Elphinstone's map exhibits the Indus holding the same course along the table land of Thibet, and receiving the Ladauk in the very same manner that they understood the Ganges to do. We have therefore the testimony of these persons, that there exists a great chain of mountains in the very quarter, and running in the very direction, indicated by Ptolemy.

In 1725, the missionaries Desideri and Freyre made the journey from Cashmire to Ladauk *. They describe it to be

* Lettres Edifiantes, vol. xi.

a succession of hardships, perils, and hair-breadth escapes, such as never mortals before encountered. Whether the worthy missionaries might somewhat exaggerate perils through which they themselves passed, I shall not inquire; but we have also the report of an Indian traveller to Mr ELPHINSTONE, that a little beyond the Cashmirian frontier, there began an uninterrupted, and, in some places, a very steep ascent, of thirteen days, to Ladauk. That place itself is described by the missionaries, as the abode of almost perpetual winter, and all the hills around covered with snow. Yet the river being already of considerable magnitude, this can only be a lower stage of the great eminence from which it descends. Ladauk, too, being the established line of communication between Great and Little Thibet, must be the most level one, and the mountains must present, at every other point, a still loftier barrier.

There appears, thus, no reason to doubt, that the Ridge of Imaus exists, and in the very region where it is placed by PTOLEMY. Up to this point, therefore, his description of Central Asia, taken in its simple and obvious sense, proves to be consistent with itself, and with the ascertained features of the region which it professes to delineate.

Having passed this formidable barrier, we arrive at the extensive region of *Scythia extra Imaum*. Upon the data now stated, this can only be Great Thibet, with an extent of Tartary stretching indefinitely northwards. After Scythia comes the famous Serica, the ultimate object of inquiry, the remotest country known to the ancients. If *Scythia extra Imaum* be Great Thibet, the next great country must be *China*. But as this is a point so curious and so much contested, it may be necessary to examine, whether the inference derived from the general line of PTOLEMY's course through Asia is supported by

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his view of the situation and geographic relations of Serica itself.

The first proof to which we shall attend is that derived from his longitudes. These are universally allowed to carry Serica very far beyond Thibet or Eastern Tartary. M. GOSSELIN, however, insists, that as they carry it beyond China also, and into the very heart of the Pacific, no regard whatever can be paid to them. That eminent writer, however, has here remarkably overlooked his own important illustrations of the mathematical geography of PTOLEMY. He has shewn, that the errors of that ancient writer, whether they reside in the value of the itinerary stadium, or, as I rather suspect, in the measurement of the great circle of the earth, are not random errors; that they proceed in a regular train, and that there exist data by which they may be calculated. It appears, that by reducing his longitudes in the relation of seven to five, they will all approximate pretty nearly to the truth. Now the remotest longitude given in Serica is 180 degrees east from the supposed meridian of the Fortunate Islands, which was fixed 2½ degrees to the west of Cape St Vincent. 180°, reduced in the above ratio, will give 128° 30′, or 117° from the meridian of London, which is three or four within the eastern frontier of China. It thus falls short of the ocean, which will account for the only circumstance ascribed to Serica which does not agree with China, that of its being bounded on the east by unknown lands. The truth is, a bounding *terra incognita* was quite a theory of the school of MARINUS, which they everywhere made to succeed to the original theory of a circumambient ocean. If the narrow peninsula of Malacca was considered as stretching into an unknown extent of continent, much more might this be supposed of an empire so vast as China, and of which the interior was so little explored. But MELA, PLINY, and in

in general, all writers not of this school, who treat of Serica, attest the general belief of antiquity, that it was bounded by an ocean on the east. PLINY even calls it the Seric Ocean.

The extent of Serica, estimated according to the graduation of PTOLEMY, will be found to be about fourteen hundred miles from north to south, and eleven hundred from east to west; which comes wonderfully near to the actual dimensions of modern China.

PTOLEMY represents Serica as traversed by two great rivers. It is impossible, with our slender information, to bring home the details of these rivers. But the fact is, that there are just two great rivers in China. These, too, for a considerable time after entering the empire, flow north-west, like the rivers of Serica; and it deserves notice, that there are no great rivers in this part of Asia, except these two, which flow in such a direction.

Let us now examine the geographical relations of Serica to the neighbouring countries. This is of course to be done on the principle which seems fully established by M. GOSSELIN, that the country of the Sinæ is Siam; in consequence of which, India beyond the Ganges is limited chiefly to the Birman empire. PTOLEMY states, that all the natives of India whom he met with in the ports of Egypt, assured him that the Seres were beyond the Sinæ. This description will apply to no country of Asia except China. He states, that India beyond the Ganges is bounded on the north, partly by Scythia, and partly by Serica. Ava, accordingly, is bounded, on that side, partly by Thibet, and partly by the Chinese province of Yunan. Again, the Sinæ are bounded on the north by Serica alone. Siam, accordingly, is so bounded by China; for the small intervening kingdom of Laos would of course be included in one or the other. Again, *Scythia extra Imaum*, is bounded

on the south by India beyond the Ganges *alone*, not at any point by India within the Ganges. *Scythia extra Imaum*, then, is entirely east of Indostan, while Serica is east of *Scythia extra Imaum*. It is needless to say to what country such a description would apply, and how inconsistent it is with the idea which makes Serica border upon, or make part of Indostan.

To these very precise statements of PTOLEMY, it may be proper to add those, though much looser and more vague, of PLINY and AMMIANUS. PLINY states, that the last country distinctly known, *(ubi gentes plane constent,)* was the chain of mountains called the Emodus. The moderns seem always to imagine, that Imaus and Emodus are merely different names for the same chain. But though instances may be found of their being so used, these never occur in the writings of the more accurate geographers. With them Emodus is east of Imaus, or rather, perhaps, it is the same chain after passing the head of the Ganges. This distinction is made by PTOLEMY in the most precise manner. With him Imaus separates India from the Sacæ, Emodus from Scythia extra Imaum. PLINY also repeatedly enumerates Imaus, Emodus, Caucasus, Parapomisus, as at least separate parts of the great chain which traverses Asia. After enumerating the nations between the Indus and the Ganges, he enumerates those between the Ganges and the Emodus. His Emodus, then, like that of PTOLEMY, is evidently a chain east of the Ganges. The great region, then, which lies east of the Emodus, cannot well be any other than China. It appears to me that PLINY confounded under the name of Seres all the nations who dwelt beyond India, as the author of the Periplus confounds them all under the name of Sinæ. I know not whether Lanos can be pronounced to be Laos; but the *Chryse Promontorum* can scarcely be any thing else than the

the Golden Chersonese. This will account for the assertion, that the people of Taprobane were placed at a small distance from, and carried on traffic with Serica.

We have thus found, that the leading astronomical and geographical features ascribed to Serica, agree exactly with China, and can apply to no other country. The moral features are, if possible, still more decisive. These do not enter into the plan of PTOLEMY, but they are noticed by PLINY, and detailed at some length by AMMIANUS. The Seres are represented as a people frugal, quiet, sedate, and tranquil beyond all others; as, of all nations the most unwarlike, and the most averse to the use of arms; as shunning, with the most studious care, the society and intercourse of other nations, and scarcely ever allowing them to enter their territory; as carrying on trade at a fixed frontier station only, and under the strictest precautions; as selling their own commodities, without receiving the commodities of other nations in return *. This

* It may be proper here to notice a report which militates against our view of the subject. Some authors describe the Seres as a people remarkable for their honesty. Thus MELA says, "gens plenum justitiæ, ex commercio, quod re-"bus in solitudine relictis absens peragit, notissimum." I think it evident, that this character is solely founded upon the rumour so often repeated, that they and their neighbours carried on trade without meeting, but by merely laying down the goods in each others absence. It is remarkable, that rumours of a trade so conducted have been transmitted in all ages, from the remote extremities of the known world, without having been confirmed in any instance by the testimony of a credible eye-witness. This inclines me to believe that it is a mere poetical fable, taking refuge, like other fables, at the dim boundaries of knowledge. While such descriptions were afloat, the quiet and cautious habits of the Seres would very naturally cause the application to be made to them. If such a trade did exist, it must have been a political precaution; and, in that case, public authority would enforce that fair-dealing, without which it could not subsist. We may finally remark, that

This is exactly the picture of the modern Chinese; and it is one totally inapplicable to any other nation of any age. If there are any from whose description it is most peculiarly remote, these certainly are the rude tribes who inhabit Tartary, Thibet, and the mountainous districts to the north of India. It is a very remarkable phenomenon in the history of man, and one which Southern Asia alone can present, that a description written nearly two thousand years ago, should paint this celebrated people with as much precision, as if it had been composed by a writer of the present day.

If, then, Serica be considered as China, and the divisions of Central Asia be regulated accordingly, then these divisions, in all their grand outlines of extent, geographical features, relative position as to themselves, and to the kingdoms of Southern Asia, will correspond exactly to the delineation of PTOLEMY. Even in the moral and political features of the remotest of these countries, there is found the most surprising correspondence. Nothing, therefore, remains to complete the proof, but to consider some details, which appear, at first sight, to militate against this supposition.

D'ANVILLE remarks, that the general aspect of Serica, as exhibited by PTOLEMY, is that of a country similar to Scythia. The truth of this observation cannot be denied. There is nothing in his description of the interior of Serica, which suggests the idea of modern China. On the contrary, there is the broad fact,

that this character is ascribed to the Seres only by secondary writers, and that PLINY and AMMIANUS, our best and most copious authorities, make no mention of such a feature. AMMIANUS, indeed, mentions the mode of trade above alluded to; but he omits entirely to draw from it the inference which is made by MELA.

fact, that it is represented as traversed by chains of mountains, a feature which the latter region does not present. This is a very important difficulty, and will require some consideration.

It is not easy for us to ascertain the degree of correctness with which Ptolemy has described Great Thibet, from our being ourselves almost totally ignorant of that region. We can only form a favourable presumption, from the accuracy with which he has delineated those conterminous regions, with which we are better acquainted *. But it is certain, that on approaching the confines of Serica, that twilight of knowledge must have begun, which always precedes its total extinction. A peculiar source of obscurity there arose. It was the decided policy of that country, as it has always been of modern China, to shut the empire, with the most jealous care, against the entrance of foreigners. Commerce was permitted only at one single point, and it was carried on with the least possible communication; nay, according to an often repeated assertion, without the parties even seeing each other. It was impossible, therefore, that the ancient writers could possess authentic details relative to the interior of Serica. But an infirmity beset Ptolemy, which continued always to prevail among geographers, till the great reform effected by D'Anville. It was conceived discreditable, that any district within the limits of [illegible] the

* I shall mention, however, the following names, in which the resemblance is somewhat rude; but allowance must be made for the passage through Grecian organs; and the positions correspond very precisely. *Chauranei* (juxta Emodos Montes), Mount Chumularee. *Achassa*,—Lassa. *Chatæ* (*Khatai*, Gr.) Khata, on the Upper Barraimpooter. (See M. Vansittart's Account of Assam. Asiat. Research. vol. vii. *Damna*,—Daum. *Ottorocoras*,—Uttarcul or Ootrecole. This last must be understood in connection with the observations which follow in the text.

the known world, should appear on the map as an entire blank. As they were far, however, from being all perfectly known, it became often necessary, in acting upon this principle, to have recourse to somewhat arbitrary measures. One of the most common was, by extension or repetition, to fill up the unknown parts out of the known, a process of which many instances must have occurred, to those who are versant in such researches. The combined celebrity and obscurity of the present object, might seem to authorise more than the usual licence. Accordingly, it appears to me, that Ptolemy has entirely protracted, into Serica, the geographical lines of Eastern Scythia. Of this operation there appear, in the very description, to be pretty evident traces. We have Issedon Scythica, Issedon Serica; while, of the Casian, Auzacian, and Emodian mountains and regions, the western part is in Scythia, and the eastern in Serica. It is, I think, quite unexampled, that any great country, much less so immense a country as Serica, and one separated by such powerful barriers from the rest of the world, should consist almost entirely of districts protracted out of another region, with which it has no natural or political connexion. We may observe, that Ptolemy stands alone in representing Serica to be a country of mountains. Ammianus, as a historian, has given, at some length, a description of its general aspect. His representation decidedly is, that of a plain of vast extent and luxuriant fertility, and which was only surrounded by a circuit of mountains. I think it even appears, that the same idea was in Ptolemy's mind; for he begins by saying, "Mountains *surround* the Seres;" though in the delineation he certainly never reaches beyond these mountains.

From the whole of what has now been said, it may be inferred, that all the details given respecting Serica are to be sought, and may perhaps be hereafter found, with some degree of precision,

cision, in the eastern extremity of Great Thibet. But the geographical outline, the broad line of distinction always drawn between Serica and Scythia, the general aspect of the former, and every thing that is said respecting its inhabitants, leave no room to doubt, that the Serica of the ancients, the country of silk, the abode of this peculiar and singular people, could be no other than the modern China. Even the short notices which are given respecting it, seem sufficient clearly to evince, that in arts and civilization, and in its whole moral and political aspect, this celebrated region was then as nearly as possible the same that it exists at the present day.

I have thus exhibited at some length that interpretation of the system of PTOLEMY and his cotemporaries, which appears to me alone consistent with their statements, and with the real aspect of those vast regions to which they refer. As, however, opinions so opposite are entertained by the most eminent geographers of the past and present age, it may be necessary, before concluding, to inquire, whether their statements exhibit the same correspondence with the ancient descriptions. D'ANVILLE here claims the precedence. He, as formerly noticed, places Serica in Eastern Tartary, north of the Great Desert, and extends it from the country of the Eygurs to the north-west extremity of China. In this system, Major RENNELL has declared his almost unqualified acquiescence; so that it is supported by very high authority indeed. Yet I think it will appear, on a careful inspection, that it breaks up completely all the relations established by PTOLEMY between Central and Southern Asia. It separates the Sacæ from Indostan, the Extra-Scythians from India beyond the Ganges, and the Seres from Siam, by an immeasurable distance. Besides, it is admitted on all hands, that the ascent of the Montes Comedorum (the Beloor) must be considerably south of Cashgar, to reach which, the caravans must have proceeded almost directly north.

north. On the contrary, PTOLEMY mentions very expressly, that, after ascending the Beloor, they turned to the south-east, and continued for more than two hundred miles in that direction. Unless, therefore, they had mistaken north for south, it was impossible they could ever arrive at Cashgar. It is only on a superficial view that this system can appear to be favoured by the graduation of PTOLEMY. From the influence of the same causes which we have noticed as acting upon the longitudes, all his latitudes in Central Asia are greatly too high. M. GOSSELIN, however, has proved, that the errors of PTOLEMY's graduation become great only by accumulating along an extensive line, and that it expresses, with tolerable correctness, the relation between two places at a moderate distance from each other. The source of the Ganges, and, with slight variations, all the points along the northern boundary line of India, are placed by PTOLEMY in latitude 37°, (about 6° too high). Now, one point in the caravan-route through the *Sacarum Regio*, is only 39°, and the highest is 43° ; being six of PTOLEMY's degrees, less than five of ours, north from the source of the Ganges. LADAUK is six ; so that in fact this statement combines with the other indications, in fixing down the country of the Sacæ to Little Thibet.

In corroboration of these proofs, it may be added, that silk is not the product of this region, and that we should look there in vain for the vast and fruitful plains described by AMMIANUS. Much less is it probable that the Seres, a mild, timid, unwarlike people, should ever have inhabited the country of the Huns and the Moguls, whose hordes have in all ages spread desolation over the eastern world.

The only ground, so far as I can discover, upon which this system has rested, is *one name*, the influence of which upon the

the geography of Central Asia, has been so remarkable, that it may require a little consideration. It has, I think, been considered unquestionable by all modern geographers, that the Casia Regio of Ptolemy can be no other than the modern Cashgar. Not only has this been the sole prop of D'Anville's hypothesis, but it has perhaps been the main source of the pertinacity, with which the Beloor has been always identified with the Imaus. I must here premise, that no proof has been used in a more irregular and licentious manner, than the one in question. The ever rolling tide of war and revolution, has swept from almost every region the names borne by it during the age of which we are treating. If we make an exception of great natural objects, mountains and rivers, there is not perhaps one name in twenty which bears any resemblance; and it is quite as likely, that this one should be the result of accident as of real identity. I would therefore lay down a principle which will scarcely perhaps be dissented from by any who have had the least experience in such researches: Where the descriptions correspond, a similar name may be considered as fixing the spot with greater certainty and precision: Where there is no such correspondence, a mere name can never be set against those grand features of nature which, remaining the same from age to age, form our only sure guide in such an investigation.

The Casia Regio is a territory of Scythia extra Imaum, bordering upon, and even, according to Ptolemy, extending into Serica. Cashgar is at an immense distance from China or Great Thibet; it is situated close to the Beloor. It would be a mere repetition of every thing that has been said, to prove that the identifying of the two breaks up the whole scheme of Pyolemy's geography of Central Asia; and that, according to him, Cashgar ought to form part of *Scythia intra Imaum*. There is a still more decided and tangible discrepancy. Cashgar

gar is situated very near to the head of the Jaxartes; the Casia Regio is immensely distant from any part of that river. Now I submit to the learned, whether the imperfect resemblance of a single name can be sufficient to overthrow, not only the whole general description of Asia, but the particular description of the very country to which it is applied. It happens very accidentally, however, that we stand here on much stronger ground; for it is surprising, that geographers should have overlooked that PTOLEMY has in another place given precisely both the name and situation of Cashgar. It is in a people of *Scythia intra Imaum*, the *Cachagæ* Scythæ, whose situation is not precisely laid down, but it is mentioned, that they lie near to the Jaxartæ, the nation who inhabit the banks of the Jaxartes. I conceive, therefore, that every argument founded upon this name must fall to the ground; after which there will not remain a single prop on which this system can rest.

Mr PINKERTON's hypothesis, which places Serica in Little Bucharia, seems liable to all the objections already urged against that of D'ANVILLE, to a somewhat greater extent, and with the addition, that he disregards entirely the dimensions assigned by PTOLEMY to those regions.

M. GOSSELIN has formed a very different system. He conceives the north of India, with the contiguous portion of Thibet, to be the real Serica of the ancients. To reconcile this with the statements of PTOLEMY, he supposes this geographer to have committed an error, when, in extending the caravan route beyond the *Purgos Lithinos*, he gave it an eastern direction. It ought, he conceives, to have been southern, which, instead of carrying the travellers towards China, would have brought them directly to the north of India. Now, I am fully aware, that the ancients erred often very materially in what the French call *orienting* their lines; that is, in giving them a proper

per direction as to the points of the compass. But I think there is scarcely an example, that so extensive a line, reaching across nearly half a continent, should be so entirely changed. I believe also that the error occurs chiefly in vague boundary lines, or in coasts, where the vessel, according to ancient practice, followed all the windings of the shore, and was acted upon by various tides and currents. It might then be difficult to ascertain the general line of their course, and an erroneous idea, once formed, might not readily be corrected. But a land-route is in a much more direct line; and as travellers have an obvious interest to know the direction in which they are moving, so the most superficial observation of the celestial phenomena will enable them to avoid any error of great magnitude. Ptolemy, we may observe, enumerates very particularly all the changes of direction made by the great caravan in its course through Asia. From the passage of the Euphrates to Hecatompylos, the capital of Parthia, he makes it east; which that line very nearly is. Thence to the capital of Hyrcania, north; that is, north-east, as east is always understood to be the general direction. Then to the capital of Margiana, by a circuitous route through Aria, first south, and then north; to Bactra, east; to the ascent of the Montes Comedorum, north-east; which agrees with Mr Elphinstone's map; then south-east, being the direction of the plain of Little Thibet; then again north-east, which is the direction of the valley of the Ladauk, the established line of communication with Great Thibet. Thus, along the whole of this immense line, the minutest variations are clearly recognised; and the improbability is greatly increased, that, in the next stage, so extraordinary an error should be committed.

The merchants farther informed Marinus, that, lengthened as the march now described had been, it formed scarcely half of the peregrination to Sera; that from the *Purgos Lithinos*, commenced

menced a laborious journey to that capital, which could not be performed in less than seven months. Admitting, with PTOLEMY, that a large allowance is to be made for the delays and difficulties of the journey, and perhaps for some degree of wanton and boastful exaggeration, there will remain enough to carry them nearly to the extremity of Asia; and it will remain quite unaccountable, how any period approaching to the above, could be spent in travelling to a place which could not be farther distant than five or six hundred miles.

If the distance and direction correspond thus ill, the description is, if possible, still more discordant. So far was PTOLEMY from the remotest idea of Sera being in India, or near the Ganges, that he fixes it about forty degrees, nearly half the length of Asia, east from that river. The most profound ignorance of this quarter of India would be necessary to account for so unheard of an error. On the contrary, we have found this region, which gives rise to the Ganges and its tributaries, to be one of those in which PTOLEMY has displayed the most decided superiority to modern information. In representing the heads of the Ganges by the rivers of Serica, PTOLEMY would have committed a very extraordinary error indeed; for these run south-west, while the others are made to run north-east; a change of direction quite unheard of. With regard to the geographical relations of Serica, it is needless to mention them, as it is evident that all these, by the present system, must be entirely thrown up.

This system, like the other, rests solely, I apprehend, upon *nominal* arguments. Some writers mention Serinda, or India Serica, a country in the north of Indostan, which actually produced silk, the staple of Serica. What is more, there are two, Stephen of Byzantium, and the anonymous geographer of Ravenna, who appear to consider this as *the* Serica, and the Seres

as Indians. In order to estimate these authorities, however, we must consider their dates. It is not till two or three centuries after Pliny and Ptolemy, that the name of *Serindi* occurs; nor is it till the sixth and eighth centuries, that these come to be confounded with the Seres. The decline of the Roman empire, and the irruption of the Turcoman hordes, broke off this grand line of commerce across Asia; silk, then, which had become the luxury of all ranks, was again sold for its weight in gold. The consequence was, that the geography of this part of the world, more than shared the general eclipse in which all branches of science were involved. Of this, the statements of the Ravenna geographer afford the clearest evidence. He evidently knew nothing of the coast of India beyond the Ganges; and the appellation of *India Serica* comprehends, with him, not only the whole interior of Indostan, but the whole of Central and Eastern Asia, to Bactriana inclusive. It is clear, therefore, that he viewed those regions in the manner natural to ignorance, as a dim and indistinct mass, the features of which were all blended together. In regard to other parts of the world, that were at all remote, he displays errors that belong only to the infancy of science. He makes the Caspian a gulf of the Northern Ocean; he extends Britain entirely from east to west, making one extremity border on Norway, and the other on Spain. His age seems therefore to be characterised by the almost total extinction, as to all remote regions, of those geographical lights which had shone upon the age of Pliny and Ptolemy; and in preferring his authority to theirs, M. Gosselin prefers, if not darkness, at least deep twilight, to the light of day.

With regard to the origin of the name *Serinda*, it does not appear very difficult to trace. Serica was known to the ancients as the country of silk; that substance itself was called *sericum.*

sericum. When, in consequence of interrupted commerce, the empire laboured under the want of this luxury, intelligence was brought, that it was produced in a region to the north of India, from whence, in fact, the silk-worms were transported to Europe. That region might then be very naturally called India Serica or Serinda, changed by the Mahommedan conquerors to the Arabic term of *Sirhind.* But M. Gosselin himself remarks, that the very names of Serinda and India Serica imply that there is another Serica, and other Seres. These he finds on the other side of the Ganges, in Serinagur, a place which certainly presents a somewhat curious coincidence of name. *Nagur* being a common appendage to Indian names of places, and signifying chief city, *Serinagur* represents almost exactly the Sera Metropolis of Ptolemy. I must confess, that I have nothing to oppose to any one who should conceive the coincidence of a single name sufficient to invalidate all the arguments on the other side which have now been stated. I shall only add, that M. Gosselin's ideas of the territory of Serinagur, are entirely derived from those erroneous views which have been dissipated by the expedition to the source of the Ganges; the result of which, though known in this country, does not seem to have reached him previous to the publication of his last volumes. He mentions Serinagur as situated among the frontier mountains, and even stretching into Thibet. He is evidently not aware, that it is entirely enclosed within the great mountain wall of India. Captain Raper and Mr Webb notice, that on ascending a hill in its vicinity, they found themselves indeed raised to a great height above the plain of India; but on turning northward, they beheld with amazement, and almost with terror, a long succession of ranges rising over each other, while high above all, towered the eternal snows of Himalaya. That commerce should take its course across this barrier

barrier of ice and desolation, when an easy and level route existed to the coast, then crowded by western navigators, is surely not very probable.

In 1802, Serinagur was visited by Captain Hardwicke, who has given a description of it, which is confirmed by Captain Raper and Mr Webb. They represent it situated in a valley, extending about a mile and a half in every direction; the city itself three quarters of a mile in length, and about half that breadth; the houses poor, and the streets so narrow, that two persons could scarcely walk abreast. There was scarcely a mansion fit for the residence of a provincial Rajah; no splendid ruins, no monuments of antiquity; nothing which could recal the splendor of that mighty metropolis, whose fame was so widely diffused over the ancient world.

From the whole of what has been said, it seems to follow, that the interpretation now proposed, is that which naturally arises out of the statements of Ptolemy. If it be admitted as true, his delineation will still, indeed, present imperfections and errors of detail; but, in all its leading features, it will be found correct, consistent with itself, and with the actual features of the region delineated. Upon the other suppositions now prevalent, it forms confessedly a mass of the most enormous errors that ever were committed by any geographer. Between these two alternatives, there could scarcely, in any case, be room for hesitation. It can be added, however, that Ptolemy is not only a writer generally of good authority, but that he has shewn himself to possess, in respect to a long series of conterminous regions, better information than had, till very recently, been attained by the moderns. I submit, then, to the learned, whether credit should be refused to his delineation of those ulterior regions, with regard to which also he probably had access to better materials than have yet fallen to the lot of modern inquirers.

 POST-

POSTSCRIPT.

Since the above was printed, I had an opportunity of submitting it to Dr Francis Buchanan, whose extensive researches into the history and geography of India are already well known to the public, and will, we are happy to learn, appear in future publications. A long residence in Nepaul afforded him peculiar opportunities of collecting information respecting the countries bordering on the northern frontier of India. I was gratified to find, that he entirely concurred in the general views contained in this essay, and even attached less importance than I myself had done, to some of the objections which may be made to them. He was so obliging as to communicate the following additional and corrective information on several important topics.

I have stated my suspicion, that the river reported to the Chinese Lamas as the Ganges, was in reality the Indus. From Dr Buchanan's information, however, I cannot doubt that it is the Sutledge, or, more properly, Satadru (Zaradrus of Ptolemy,) which rising from the lake Manas Saroer, crosses the Snowy Chain, and rolls through the Punjaub into the Indus. Dr Buchanan is also of opinion, though it is otherwise represented by Mr Arrowsmith, that the Gogra rises from a lake near the Manas Saroer, and crosses the Himalaya.

Dr Buchanan is convinced, from positive information, of the existence of the Chain of Mountains separating Great from Little Thibet, which I have supposed to be the Northern Imaus of Ptolemy. It appears in the most recent map of Mr Arrowsmith, under the name of Mount *Caillas*, at precisely the same distance east from the source of the Ganges, that Ptolemy has placed his bend of the Imaus; and contains on its opposite sides the sources of the Barrumpooter (properly Bramapoutra,)

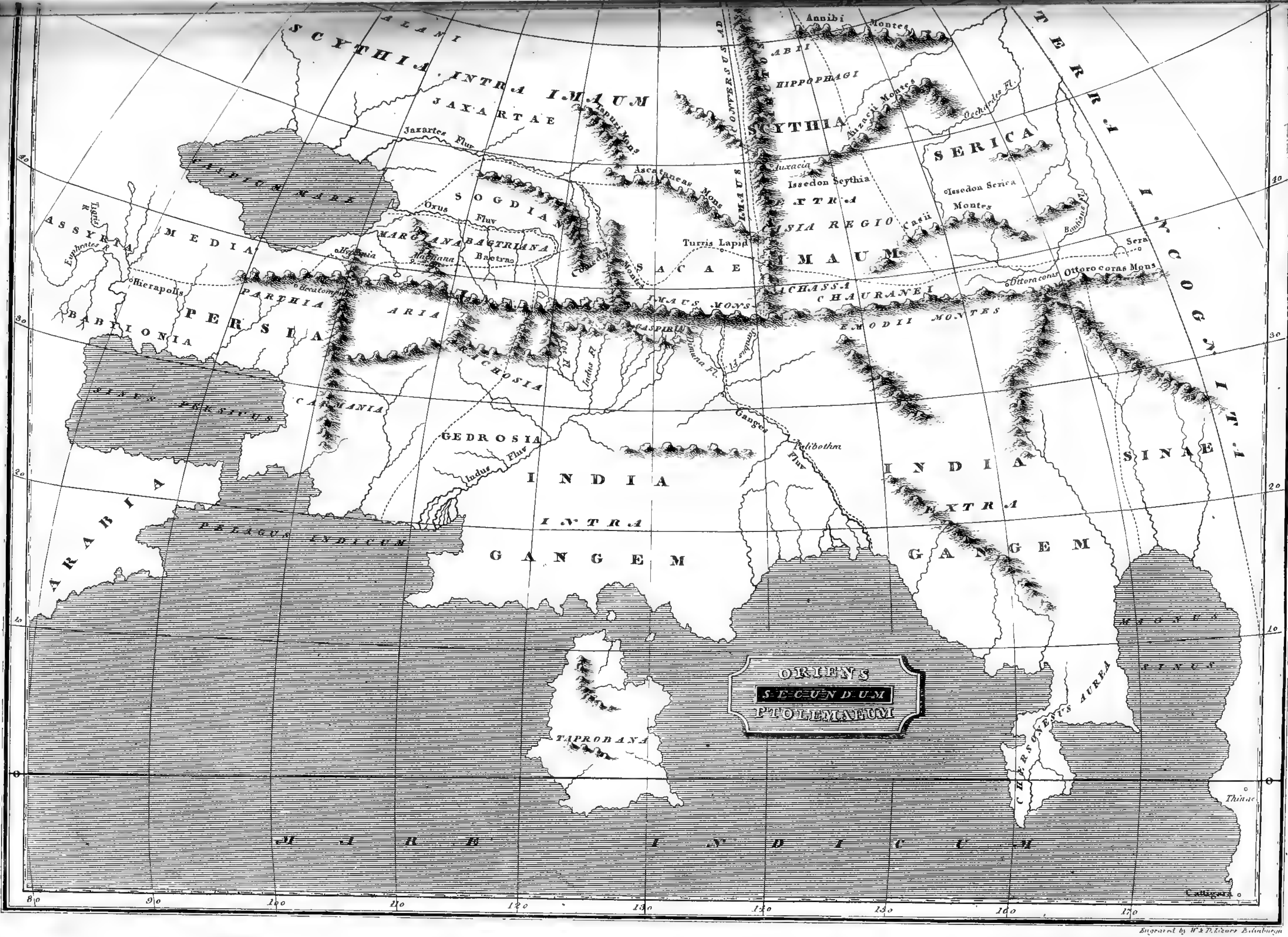
Bramapoutra,) the Satadru, and the Ladauk, which is evidently the largest branch of the Indus. Mr Arrowsmith has extended it along the western bank of the Ladauk; but Dr Buchanan has every reason to believe, that it stretches along the opposite side, and unites itself to the Mooz Taugh, forming a complete barrier between Great and Little Thibet. Whether or how far it extends northward, in the direction indicated by Ptolemy, modern geography does not afford the means of ascertaining. Dr Buchanan is disposed to view it as a prolongation of the Mooz Taugh, turning southwards, and uniting with the Himalaya. This is evidently little more than a nominal distinction from the opposite view of Ptolemy, in making it a continuation of the Imaus northward.

Under Dr Buchanan's direction, I have ventured to lay down the chain of mountains in question according to his idea of their course; also the sources of the Bramapoutra, Ladauk, Satadru and Gogra.

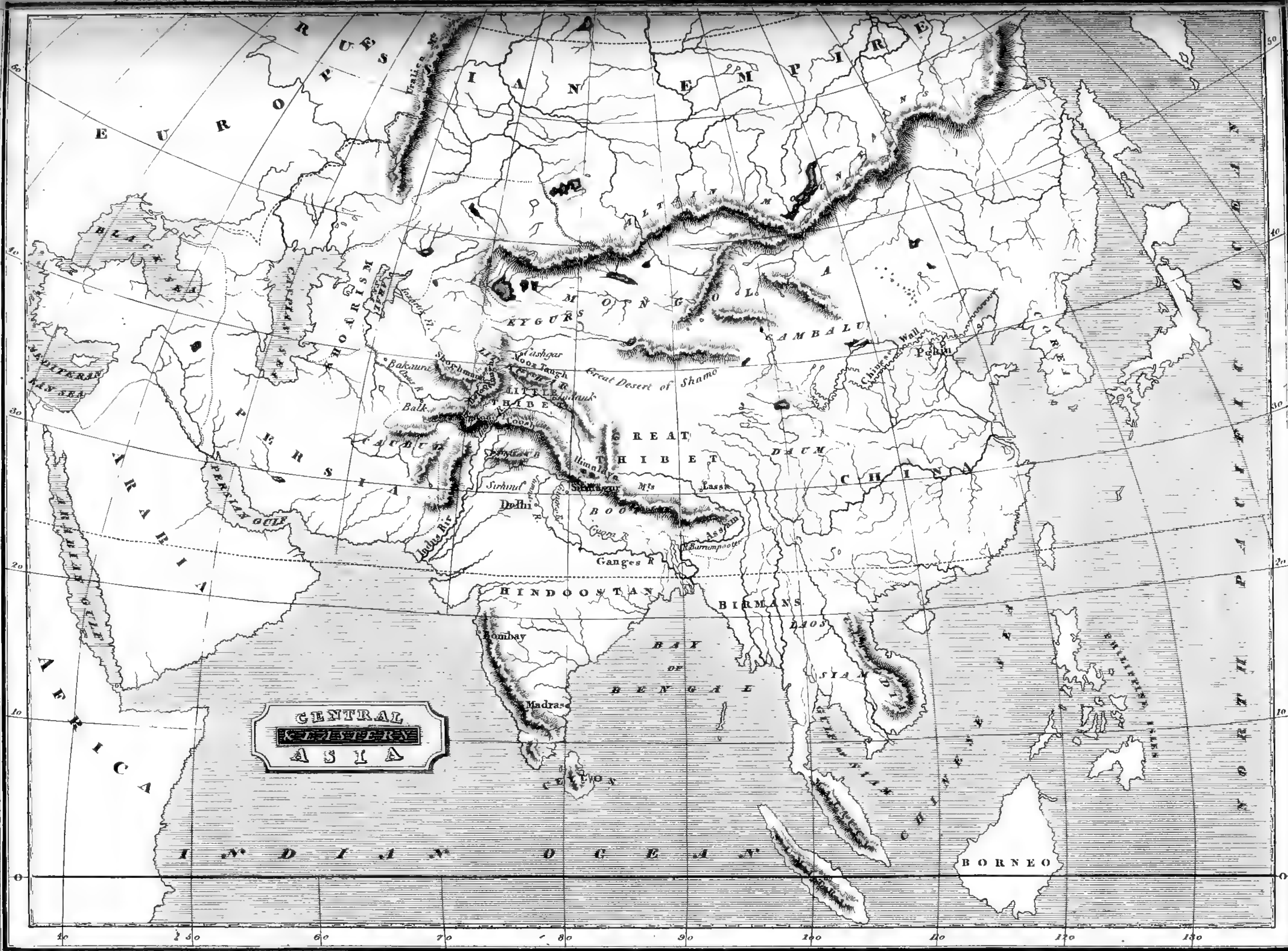
Dr Buchanan is of opinion, that the representation of China as a country of mountains would not be so wholly incorrect as I have supposed. He conceives, that the lofty chain separating Canton from the northern provinces, and which was crossed by Sir George Staunton, is a prolongation of the Himalaya. This would make Ptolemy correct, in extending that chain, under the name of *Ottorocoras*, to the extremity of the known world. The western provinces of the empire are also in general mountainous.

Dr Buchanan attaches no importance to the name of Serinagur, (properly Sreenagur, *the Holy City*,) as he knows its foundation to be entirely modern, and not to reach back above two or three centuries.

The ancient Map is copied from those usually appended to Ptolemy's Geography, unless in a few instances, where these appeared to differ from the text, which is universally allowed to be of higher authority.



ORIENS
SECUNDUM
PTOLEMAEUM
TERRA INCOGNITA
SCYTHIA INTRA IMAUM
SCYTHIA
SERICA
SINAE
INDIA EXTRA GANGEM
INDIA INTRA GANGEM
TAPROBANA
MARE INDICUM
PELAGUS INDICUM
SINUS PERSICUS
CASPIUM MARE
ARABIA
ASSYRIA
BABYLONIA
MEDIA
PERSIA
PARTHIA
ARIA
CARMANIA
GEDROSIA
ARACHOSIA
SOGDIANA
MARGIANA
BACTRIANA
SACAE
IMAUS MONS
EMODII MONTES
CHAURANEI
ACHASSA
CASPIRIA
JAXARTAE
ALANI
ABII
HIPPOPHAGI
Annibi Montes
Issedon Scythia
Issedon Serica
Casii Montes
Ottorocoras Mons
Ottorocora
Sera
Turris Lapid
Ascatancas Mons
Jaxartes Flux
Oxus Fluv
Indus Fluv
Ganges Fluv
Palibothra
Bactra
Hierapolis
Hecatompylos
Tigris
Euphrates
Coë Fl.
Indus Fl.
Diamuna Fl.
AUREA CHERSONESUS
MAGNUS SINUS
Thinae
Cattigara
Engraved by W. & D. Lizars Edinburgh



CENTRAL
& EASTERN
ASIA
RUSSIAN EMPIRE
EUROPE
MONGOLIA
EYGURS
KHOARISM
ARAL SEA
CASPIAN SEA
BLACK SEA
MEDITERRANEAN SEA
PERSIA
ARABIA
PERSIAN GULF
ARABIAN GULF
AFRICA
CAUBUL
Balk
Bakaura
Oxus R.
Kashgar
Great Desert of Shamo
LITTLE THIBET
GREAT THIBET
Himmalaya Mts
BOOTAN
Lassa
Assam
Burrampooter R.
Ganges R.
Delhi
Sirhind
Indus Riv
HINDOOSTAN
Bombay
Madras
CEYLON
BAY OF BENGAL
INDIAN OCEAN
BIRMANS
LAOS
SIAM
GULF of SIAM
CHINA
DAUM
CAMBALU
Chinese Wall
Pekin
COREA
PHILIPPINE ISLES
BORNEO
CHINA SEA
PACIFIC OCEAN
Engraved by W. & D. Lizars Edinburgh.

XI. *An Analysis of Sea-Water; with Observations on the Analysis of Salt-Brines. By* JOHN MURRAY, M. D. F. R. S. E.

(Read 15th April, and 20th May, 1816.)

THE composition of Sea-Water has been variously stated by different chemists, not only with regard to the proportions of the salts which it holds in solution, but with regard even to the ingredients themselves.

According to LAVOISIER, it contains muriate of soda, muriate of magnesia, and muriate of lime, sulphate of soda, sulphate of magnesia, sulphate and carbonate of lime. The proportions he assigns are, in a pound of water, (French weights) 126 grains of muriate of soda, $14\frac{3}{4}$ grains muriate of magnesia, 23 grains muriate of lime mixed with muriate of magnesia, 7 grains of sulphate of soda and sulphate of magnesia, and 8 grains of sulphate and carbonate of lime *.

BERGMAN gives a very different statement. He found only muriate of soda, muriate of magnesia, and sulphate of lime; the proportions in a Swedish kanne, which is equal to about $6\frac{1}{2}$ English pints, are 2 ounces 433 grains of muriate of soda, 380 grains

* Memoires de l'Academie des Sciences, 1772.

grains of muriate of magnesia, and 45 grains of sulphate of lime *. Reducing them to English weights, they are equal, in a pint of water, to, muriate of soda 241 grains, muriate of magnesia 65.5, sulphate of lime 8. This, however, is with regard to water from the Canaries, containing 1 part of saline matter in about $23\frac{1}{4}$ of water. Reducing it to the proportion of the water of our shores, that of about 1 to 30, the proportions will be, muriate of soda 186.5, muriate of magnesia 51, sulphate of lime 6 = 243.5 grains.

Bergman's analysis is evidently incorrect in the omission of sulphate of magnesia, which every other chemist has obtained, and which is known to be extracted even on a large scale. And, what is singular, this did not arise from his not being aware that it might be present. On the contrary, he made an experiment to discover it; and even now, in reviewing his method, it is not apparent how he had been deceived. He evaporated to dryness, and treated the dry residuum with alcohol, by which he found muriate of magnesia to be dissolved; he then washed the residual matter, consisting chiefly of muriate of soda, with a small quantity of warm water, by which, as he remarked, if any sulphate of magnesia were present, it ought to have been dissolved. But this water shewed no signs of the presence of this salt, either in taste or by precipitation, and contained nothing but a small portion of common salt. Now unquestionably, in this way, sulphate of magnesia ought to have been discovered; or if it should be supposed that it does not originally exist, but that sulphate of soda is the primary ingredient, still the method employed was equally proper to discover this latter salt. The only supposition that can be made is, that, in the first step of the analysis, a very weak alcohol

* Bergman's Essays, vol. i. p. 230.

hol had been used in large quantity, by which a portion of these sulphates would be dissolved, though still it is difficult to imagine that in this way they would be entirely abstracted.

LAVOISIER's analysis has been considered as incorrect in two circumstances,—in the finding muriate of lime and sulphate of soda. Neither of these have been discovered by other chemists; and in a late analysis of sea-water by VOGEL and LAGRANGE, one of the objects of experiment was to detect their presence, and the conclusions drawn were, "that sea-water "contains no sulphate of soda," and "no muriate of lime." In this analysis the saline ingredients found in sea-water were the same as those assigned by BERGMAN, with the addition of sulphate of magnesia. In 1000 grammes there were found 25.10 grammes of muriate of soda, 3.5 of muriate of magnesia, 5.78 of sulphate of magnesia, 0.20 of carbonate of lime and magnesia, and 0.15 of sulphate of lime *.

Some other recent analyses have been given; that by LICHTENBERG is noticed by VOGEL and LAGRANGE, from a German Journal, as approaching to their own; and that of PFAFF, in which, as in LAVOISIER's analysis, there is found a portion of muriate of lime.

It is obvious, that there remains a degree of uncertainty with regard to the ingredients of sea-water, sufficient to give interest to a new analysis. The principle, too, which I have illustrated in a preceding paper, on the analysis of Mineral Waters,—that the substances obtained are not always to be regarded as the original ingredients, but frequently as products of new combinations established by the analytic operations, may contribute to throw light on the conclusions to be drawn, and seemed

* THOMSON's Annals, vol. iv. p. 200.

seemed to me to admit of being applied to the explanation of some of the preceding results. This led to the experiments of which I now propose to give an account.

The peculiarity in the results of Lavoisier's analysis, and with regard to which the others differ from him, is the obtaining, as ingredients of sea-water, portions of sulphate of soda and muriate of lime. Applying the principle now referred to, it is obvious, that in an analysis by evaporation, the composition of these salts would be subverted by their reciprocal action; neither of them would be obtained; but by mutual decomposition they would be converted into muriate of soda, and sulphate of lime. Sulphate of lime is accordingly obtained in all these analyses, and probably has this origin.

But, admitting this, how had muriate of lime, and sulphate of soda, been procured by Lavoisier. This, supposing the result accurate, can only be ascribed to some peculiarity in his process, by which their mutual action had been prevented, and their distinct existence preserved. The method he employed was to evaporate sea-water to dryness; during the evaporation, sulphate and carbonate of lime were precipitated and were withdrawn; the dry saline mass was lixiviated with alcohol; and the ley being poured off clear, was found to hold in solution muriate of magnesia, and muriate of lime; the undissolved matter was then heated, with a mixture of two parts of alcohol and one of water, by which it was almost entirely dissolved; it deposited, however, on cooling, a white powder, which was found to be sulphate of soda, and sulphate of magnesia, and it retained dissolved the muriate of soda of the sea-water with a portion likewise of muriate of magnesia.

Now a portion of sulphate of lime was obtained in this process, which, according to the view I have stated, was probably produced

produced by the mutual decomposition of sulphate of soda and muriate of lime. But it is also possible, that this decomposition might not be complete. I had formerly found, indeed, that when a liquor containing these two salts is evaporated, their decomposition is not entirely effected *; it seemed possible, therefore, that portions of both might remain undecomposed in Lavoisier's process; the alcohol applied to the solid matter would remove the muriate of lime, and thus the sulphate of soda would remain. To elucidate the whole subject, therefore, it seemed best to repeat Lavoisier's analysis as he had performed it, and ascertain the actual results.

A. Four pints of sea-water of the Frith of Forth, taken up near Leith, at a distance from any fresh water, were evaporated by the heat of a sand-bath; the evaporation being continued until a pellicle of salt formed on the boiling liquor. A precipitate subsided during the boiling, which being washed, weighed when dry 25 grains.

B. The liquor was evaporated to dryness, and the saline mass was dried thoroughly by a continued heat of about 150°; it weighed 1025 grains. To separate the salts composing it, it was submitted to the action of alcohol. About 4 ounces of alcohol of the specific gravity of 840 were poured upon it in a bottle, and allowed to remain over it for 12 hours, being occasionally agitated; and when poured off an ounce of the same alcohol was added, and after frequent agitation, and being kept over it for some hours, was poured off, and added to the former.

C. The residuum, when dried, weighed 890 grains; 135 grains had therefore been abstracted, consisting chiefly of earthy muriates.

* Transactions, Vol. vii p. 475.

D. The saline matter was digested with 9 ounces of a weaker spirit, composed of 2 of alcohol and 1 of water, heat being applied to it by a sand-bath nearly to ebullition, with frequent agitation; and the liquor having been poured off while hot, 4 ounces more of the same diluted alcohol were added, heated as before, and after it had become clear by subsidence, this liquor was added to the other. The greater part of the saline mass, consisting chiefly of muriate of soda, was thus dissolved.

E. The residue was submitted to the action of successive quantities of a still weaker spirit, composed of 3 of alkohol, and 4 of water, aided by heat, with the view of dissolving the sulphate of magnesia and of soda. A solution was obtained of a strong saline taste.

F. To abstract these salts more completely, the residue was lixiviated with small successive portions of warm water; a solution having a similar taste was obtained.

G. There was left at length a powder, soft, light, tasteless, and insoluble.

It now remained to examine these products more minutely, to determine their nature, and estimate precisely their quantities.

The powder obtained in the first evaporation A, consists, according to Lavoisier, of sulphate and carbonate of lime. It weighed when dry 25 grains; it was submitted to the action of a very dilute alcohol, acidulated with muriatic acid, which excited effervescence; this being poured off, and the residue being lixiviated, and dried, weighed 22 grains. It was *sulphate of lime*, and absorbed water with avidity, becoming solid and dry. The liquor poured off, afforded by evaporation a saline deliquescent matter, which, heated with sulphuric acid, gave products

products equivalent to 1.7 grains of *carbonate* of *magnesia*, and 1.2 grain of *carbonate* of *lime.*

The solution (B) obtained by the action of the stronger alcohol, ought to have contained, according to the results of Lavoisier's analysis, muriate of magnesia and muriate of lime. A small portion of it was diluted with distilled water, and a few drops of a solution of oxalate of ammonia were added, but caused no precipitation, nor even any opacity. The liquor, therefore, contained no muriate of lime. It was distilled to dryness. The dry matter deliquesced on exposure to the air; being lixiviated with alcohol, a small portion of muriate of soda remained undissolved, which was added to the solution D. The liquor being evaporated so far as to be of an oily consistence, afforded, on cooling, *muriate* of *magnesia* in prisms. This, dried until it had no appearance of moisture, weighed 145 grains. Decomposed by sulphuric acid, it afforded 105.9 grains of dry sulphate of magnesia, equivalent to 88.5 of real muriate.

The solution D had a strong saline taste, and, in cooling, had deposited muriate of soda in cubes on the sides of the bottle. A little of it being diluted with distilled water, oxalate of ammonia did not impair the transparency. Carbonate of potash, and muriate of barytes, produced a turbid appearance. The entire liquor was submitted to distillation, until the alcohol was abstracted, and was then evaporated in an open bason, until crystals formed in it while hot. These were cubes of *muriate* of *soda*, and this salt continued to be afforded by successive evaporations. The last product deliquesced a little on exposure to the air, indicating the presence of muriate of magnesia; and the remaining liquor afforded by evaporation a deliquescent saline mass: both these were washed with repeated portions of alcohol; muriate of magnesia was thus obtained,

 which

which dried, weighed 17.3 grains, and which, converted into sulphate, gave 12.4 grains, equivalent to 9.7 of real muriate. The matter not dissolved by the alcohol, being dissolved in water, afforded by slow evaporation sulphate of magnesia in prisms, which dried, weighed 6.3 grains. The crystallised muriate of soda, dried at a heat of 200°, weighed 580 grains.

The solution E deposited, on standing after twelve hours, crystals in flat striated prisms, having every appearance of *sulphate* of *soda*, and which, on more minute examination, were found to be so : freed from sensible moisture, they weighed 18 grains. The liquor diluted with distilled water, was not sensibly affected by oxalate of ammonia ; it became slightly turbid with subcarbonate of potash, and with muriate of barytes. The alcohol was drawn off by distillation ; being then submitted to evaporation, a crust of muriate of soda formed on the surface, and crystals in cubes were deposited ; additional portions of them were obtained by successive evaporations, and the liquor continued to afford a crust of muriate of soda on its surface, while hot, until it was almost entirely evaporated. A small portion of liquor remained, which, on cooling, afforded prismatic crystals of sulphate of magnesia, which, freed from moisture, weighed 8.9 grains. The muriate of soda dried weighed 170.8 grains.

The first portions of the aqueous solution F had deposited crystals of sulphate of soda on cooling ; and the whole quantity being partially evaporated, yielded an additional portion. The crystals of both, freed from adhering moisture, weighed 44.2 grains. The liquor being farther evaporated, cubes of muriate of soda were formed on the sides of the capsule, while it was warm, and by continuing the evaporation, a quantity of this salt was obtained, which weighed when dry 12.3 grains.

A

A small portion of liquor remained, which, by farther evaporation, yielded crystals of sulphate of soda to the amount of 6 grains, with crystals of muriate of soda 2 grains.

The portions of muriate of soda obtained in the preceding experiments amounted to 765.1 grains. None of them, however, were perfectly pure. Their solutions became turbid on the addition of sub-carbonate of soda, and of muriate of barytes, indicating the presence of sulphate of magnesia, or of muriate of magnesia and sulphate of soda, and probably indeed of portions of all these. The whole was submitted to the action of highly rectified alcohol for 12 hours, with repeated agitation; the alcohol acquired a bitter taste; being poured off, and distilled, it afforded muriate of magnesia, which, heated with sulphuric acid, gave a product equivalent to 6.2 of real muriate. The residual salt still gave indications of the presence of sulphate of magnesia, by the tests of muriate of barytes and sub-carbonate of soda. The difficulty is so great, of separating a small portion of a salt from a large quantity of another, where the difference in their solubility is not considerable, that instead of attempting to remove the sulphate of magnesia by farther crystallizations, it was decomposed by adding to the solution sub-carbonate of soda; the precipitate was collected, and converted into sulphate of magnesia by the addition of sulphuric acid. This, dried at a low red-heat, weighed 16 grains, equivalent to 33 crystallised. The salt had been previously exposed to a red-heat, when it weighed 744.5 grains. The above quantity of sulphate abstracted from this, leaves as the real quantity of muriate of soda 728.5 grains.

The powder G was soft, light, and tasteless. It weighed when thoroughly dried 7.5 grains. It might be expected to be similar to the powder A, and was therefore subjected to the same treatment. Diluted alcohol, acidulated with muriatic acid,

acid, excited effervescence; the liquor poured off afforded by evaporation muriates equivalent to 2.8 of *carbonate* of *magnesia*, and 1.3 of *carbonate* of *lime*. And the remaining *sulphate* of *lime* dried weighed 3 grains.

By this analysis, then, the substances obtained from 4 pints of sea-water, and their proportions, are as follow:

Muriate of Soda, - -		728.5 grains.
Muriate of Magnesia real,	83.5	
	9.7	
	6.2	
	99.4	99.4
Sulphate of Magnesia crystallised, - -	6.3	
	8.9	
	33	
	48.2, or real,	23.5
Sulphate of Soda crystallised,	18	
	44.2	
	6	
	68.2, or real,	30.2
Sulphate of Lime, real,	22	
	3	
	25	25
Carbonate of Lime, -	1.2	
	1.3	
	2.5	2.5

Carbonate

Carbonate of Magnesia,	1.7	
	2.8	
	4.5	4.5

The two last ingredients might be accidental products, from the decomposition of muriate of magnesia and of lime. Muriate of magnesia is decomposed by heat; a portion of its acid is expelled; and the magnesia separated in consequence of this, will absorb carbonic acid, from the current of warm air applied during the evaporation, or from the carbonic acid gas which the sea-water itself contains, and which is not immediately expelled by heat. The small portion of carbonate of lime might be produced in a similar manner, or from the action of the carbonate of magnesia on muriate or sulphate of lime. I accordingly found, in a subsequent analysis, that on adding muriate of barytes to sea-water, no carbonate but only sulphate of barytes is precipitated, which proves that these conclusions are just. For the small portion, therefore, of carbonate of magnesia, the equivalent portion of muriate of magnesia, 4.2, raising it to 103.6, is to be substituted. If the lime which afforded the carbonate existed in the state of sulphate, then the equivalent portion of this 3.4 is to be added to the sulphate actually obtained, making it 28.4. If it existed in the state of muriate of lime, it still would, but for this change, have been converted in the progress of the evaporation into sulphate of lime; the same substitution, therefore, is in this view equally to be made. With these corrections, and reducing the proportions to a pint, the ingredients and their quantities will be as follow:

Muriate of Soda,	182.1	grains.
Muriate of Magnesia,	25.9	
Sulphate of Soda,	7.5	
Sulphate of Magnesia,	5.9	
Sulphate of Lime,	7.1	
	228.5	

The

The results of the preceding analysis are different from those I had expected to obtain. I had supposed, that in LAVOISIER's method, the sulphate of soda, and muriate of lime, which he stated as ingredients, had been obtained from some peculiarity in the process by which their reciprocal action, and consequent transition into muriate of soda and sulphate of lime had been prevented; and that in the common method of evaporation they are not obtained, because this mutual decomposition takes place. It appears, however, that the results by LAVOISIER's method are different from those he stated, and are such as preclude this view. No muriate of lime is obtained, and sulphate of lime is obtained in considerable quantity; of course, the sulphate of soda, which is also found, cannot be considered as being procured, in consequence of its decomposition by muriate of lime being prevented by any peculiarity in the process, and must therefore be ascribed to some other cause.

Besides the peculiarity in this analysis of sulphate of soda, there is another singularity in the result, that little sulphate of magnesia is procured. This salt, it is well known, is extracted in considerable quantity by the common process of evaporation of sea-water on a large scale, being obtained by boiling down the bittern, while, by this method, little or no sulphate of soda is obtained.

The products of this analysis are thus so different from those usually assigned, and so different from those known to be afforded by the usual process of evaporation, that it became desirable to perform the analysis in the common mode, so as to ascertain the actual results of it with precision, with a view to determine on what these differences depend. This I accordingly executed.

A. Four pints of the same sea-water were submitted to evaporation in a sand-bath, and after the crystallization of the muriate

riate of soda commenced, the liquor was poured off at intervals from the salt deposited, and farther evaporated. This was continued as long as it appeared to afford no other salt on cooling than muriate of soda. The latter products of this salt were less pure than the first, being deliquescent on exposure to a dry atmosphere; they were therefore redissolved in water; by evaporation, the greater part was obtained crystallized, in a purer state, and was added to the other; and the small portion of residual liquor was added to the residual liquor of the evaporated sea-water.

B. By farther evaporation, this liquor afforded crystals in slender prisms, which were permanent in the air, and which were found to be sulphate of magnesia; by repeated evaporations, successive crystallizations of this kind were produced, (small portions of muriate of soda being also obtained, which, after being washed, were added to the salt A); the products of the first crystallizations were nearly pure; those of the latter crystallizations were less distinct in form, and were in part deliquescent.

C. The portion of liquor still remaining was evaporated, until, on cooling, it formed a congeries of slender prisms, which, exposed to the air, deliquesced, and soon passed to a state of perfect solution, a proof of their being principally muriate of magnesia.

The products thus obtained, consisted, first, of muriate of soda A; secondly, of sulphate of magnesia B; and, thirdly, of muriate of magnesia C. These, however, could not be supposed to be pure, and they were, therefore, submitted to farther examination.

D. The muriate of soda A, gave indications of the intermixture of magnesian salts; the solution of a minute portion of it in distilled water becoming turbid on the addition of carbo-

nate of soda. It was also to be presumed, that there would be mixed with it any sulphate or carbonate of lime deposited during the evaporation. It was therefore re-dissolved in water. There remained undissolved a residue, which, when thoroughly dried, weighed 22.6 grains. The salt was again procured by evaporation, but it was still not perfectly pure. Its dilute solution gave a milkiness with carbonate of soda; and oxalate of ammonia and muriate of barytes rendered it turbid, indicating the presence either of a little muriate of lime with sulphate of soda, or magnesia, or of sulphate of lime with a portion of sulphate or muriate of magnesia. The whole was redissolved in distilled water, a powder similar in appearance to the insoluble residue of the former solution, remained undissolved, which, when thoroughly dried, weighed 10.3 grains. To the clear solution a portion of alcohol was added, not sufficient to cause any precipitation of muriate of soda; it produced a slight turbid appearance, and after some hours a powder had subsided, which, after being washed with water, was tasteless: it weighed 1.5 grains. The muriate of soda, obtained by evaporation, weighed, when dried, 718 grains. Being still not entirely pure, it was reserved for another operation.

E. The insoluble residues collected in the preceding operations being put together, were submitted to the action of alcohol, acidulated with muriatic acid, to remove any carbonate of lime, or of magnesia. Effervescence was excited; the liquor being poured off, and the insoluble residue of sulphate of lime being washed with a little water, weighed, after exposure to a heat nearly equal to ignition, 26.3 grains. The alcoholic solution, with the addition of the small portion of water with which the sulphate of lime had been washed, afforded, by evaporation, a matter which entered readily into fusion, and which, treated with sulphuric acid, gave 5.6 of sulphate, equivalent

valent to 4.3 of carbonate of magnesia, and 3 of sulphate, equivalent to 2.2 of carbonate of lime. I have already observed, however, that no carbonic acid is detected in sea-water by the test of barytes; these carbonates, therefore, are, as before, to be considered as products of the evaporation, arising from the decomposition of muriate of magnesia, and of muriate or sulphate of lime. The one, but for the decomposition by which it is produced, would have appeared as sulphate of lime; it increases, therefore, the proportion of that ingredient to 29.3 grains. The portion of muriate, equivalent to the other, that is, 4.4 grains, may be added to the quantity of that salt obtained in the subsequent steps of the analysis.

F. The products of the different crystallizations B, consisting chiefly of sulphate of magnesia, with portions of muriate of magnesia, were left exposed to the air for some days, and the liquor formed from them by deliquescence, was poured off occasionally, and added to the solution of muriate of magnesia, C. The residues were then washed with pure alcohol, to abstract more completely any muriate of magnesia. The portions remaining undissolved, were dissolved together in water. By evaporation, they afforded sulphate of magnesia in bevelled prisms; by farther evaporation, muriate of soda in cubes was obtained; and by successive evaporations, there were thus procured sulphate of magnesia in crystals, 46.6 grains; and muriate of soda 39 grains. A small portion of liquor remained, which, containing chiefly muriate of magnesia, was added to the liquor C.

G. This liquor C to which the portion of liquor formed by deliquescence from B, had also been added, was evaporated to dryness. It was then submitted to the action of successive portions of alcohol, employing, first, the alcohol with which the saline matter B had been lixiviated, and afterwards pure alcohol.

cohol. These liquors, poured off from a portion which remained undissolved, were evaporated to dryness; the dry mass was dissolved in water, and, by a second evaporation, afforded a congeries of prisms of *Muriate of Magnesia.* Dried by a heat of 150°, the weight amounted to 156 grains. Converted into sulphate of magnesia by the addition of sulphuric acid, the product weighed, after being dried at a low red-heat, 99.2 grains, equivalent to 78.4 of real muriate of magnesia.

H. The matter which remained undissolved by the alcohol G was dissolved in distilled water. The solution was evaporated, until, by a farther spontaneous evaporation in a warm apartment, crystals were successively formed; these were sulphate of magnesia, and, in general, bevelled prisms. The whole freed from moisture weighed 48.6 grains. A small portion of liquor remained, which, when evaporated, gave a deliquescent saline mass: by slow evaporation 2.6 grains of muriate of soda were obtained from it; the remainder yielded muriate of magnesia equal to 3 grains.

I. The crystals of sulphate of magnesia obtained by the successive evaporations, were not all equally well formed; and after they had been left exposed to a dry air for some time, some of them became quite efflorescent, while others did not. The former were picked out, and each portion was re-dissolved in water. By a new crystallization, there were thus obtained 72 grains of sulphate of magnesia, and 18.5 of sulphate of soda.

K. The muriate of soda obtained in the preceding steps, amounted in all to 759.6 grains. After exposure to a red-heat, it weighed 752.4 grains. It has already been stated, that it was not perfectly pure; its solution being rendered milky, both by sub-carbonate of soda and muriate of barytes. The separation of the sulphate of magnesia, which this chiefly indicated,

indicated, was not to be completely looked for by solution and crystallization. Sub-carbonate of soda was therefore added to the solution as long as any precipitation took place; the precipitate heated with a sufficient proportion of sulphuric acid to redness, gave 16.4 sulphate of magnesia, equivalent to 33.7 of the same salt crystallised. The former quantity abstracted from the weight of the muriate of soda, reduces it to 736 grains.

This analysis, then, affords the following ingredients, and their proportions in their *real* state.

Muriate of Soda, -	736 grains.
Muriate of Magnesia, -	85.8
Sulphate of Magnesia, -	51.2
Sulphate of Soda, -	8
Sulphate of Lime, -	29.3

Or, reducing them to a pint of the water,

Muriate of Soda, -	184 grains.
Muriate of Magnesia, -	21.5
Sulphate of Magnesia, -	12.8
Sulphate of Soda, -	2
Sulphate of Lime, -	7.3
	227.6

By the two modes of analysis now stated, different results have been obtained. There are common to both as the principal products, Muriate of Soda, and Muriate of Magnesia. But in the one, sulphate of magnesia, with only a small proportion of sulphate of soda, are procured. In the other, sulphate

phate of soda in a much larger quantity, with only an inferior proportion of sulphate of magnesia, are obtained. How is this diversity of result to be accounted for?

As the relative quantities of these salts are thus varied, and are indeed nearly altogether dependent on the kind of analysis, it is obvious that one or other of them must be an original ingredient, and the other must be a product of decomposition. If sulphate of magnesia is the original ingredient, then, when it is not obtained, or is obtained only in very inferior quantity, while sulphate of soda is procured in its place, it must be held that it is decomposed; and the only decomposition that can account for these results is that from the mutual action of muriate of soda and sulphate of magnesia, by which, while portions of them are removed, corresponding portions of sulphate of soda and of muriate of magnesia are formed. On the other hand, if sulphate of soda is the original ingredient, then, when it is not obtained, or is obtained only in small quantity, it must be held that it is decomposed; and the only decomposition of it that can here take place, must be from the action of muriate of magnesia, by which, while quantities of both these salts are removed, corresponding quantities of sulphate of magnesia and muriate of soda will be produced.

Of the two analyses, the one in which sulphate of soda principally is obtained, is that in which the solvent action of alcohol is employed; the other, in which there is the mere separation of the salts by evaporation and crystallization, is that which affords scarcely any of it, but in place of it sulphate of magnesia. Now, it is to be observed, that in both of these the preliminary operation of evaporation to dryness is the same. Since sulphate of soda, therefore, is not obtained by this operation, it is obvious, that, even on the assumption of its being the original ingredient of sea-water, it must, in the progress of the

the evaporation be decomposed by the muriate of magnesia, and converted into the sulphate of magnesia; and hence, in the subsequent solvent action of the alcohol, by which it is obtained, it must be re-formed. And, on the other hand, if sulphate of magnesia is the primary ingredient, and is obtained as such by the evaporation, it remains to be explained, how it is converted in the subsequent solution by the alcohol into sulphate of soda. The whole question, therefore, resolves itself into the nature of the action of the alcohol, producing sulphate of soda; and of this I perceive no other solution than that which I have now to illustrate.

The fact, however it is to be explained, or to be reconciled with the doctrine of chemical attraction giving rise to combinations or decompositions according to the strength with which it is exerted, seems to be established by an induction too strict and extensive to admit of doubt, that these results are often determined by the force of cohesion, in such a manner, that in principles acting on each other, those on which this force operates most powerfully, in relation to the fluid which is the medium of action, are combined together. So much is this the case, that, as BERTHOLLET has justly remarked, we may, from a knowledge of the solubility of the compounds which substances form, predict what combinations will be established when they act on each other; those always combining which form the least soluble compounds. So far the influence of this has been illustrated by this able chemist. But it appears to me to admit of farther extension, so as to afford a solution of the present question.

If the force of cohesion can so far modify chemical attraction, as to establish among compound salts dissolved in any medium, those combinations whence the least soluble compounds are formed, we are entitled, I conceive, to conclude, that

that the reverse of this force, that is, the power of a solvent, may produce the opposite effect, or cause the reverse of these combinations to be established. Suppose muriate of magnesia and sulphate of soda to be dissolved in water, and the solution to be concentrated by evaporation, the combinations of sulphate of magnesia and muriate of soda, being on the whole less soluble in water, this circumstance of inferior solubility, or the force of cohesion thus operating, may determine the formation of these, and, accordingly, their formation is found by experiment to take place. But suppose sulphate of soda and muriate of magnesia to be dissolved by the aid of heat in alcohol so far diluted as to effect their solution, then those combinations will not be established which existed in the watery solution, because, on the whole, sulphate of magnesia, and muriate of soda, are less soluble in alcohol, even in this diluted state, than sulphate of soda and muriate of magnesia. These latter compounds will, therefore, remain undecomposed. But farther, this *may* give rise, or, rather, *must* give rise, in conformity to the principle above stated, to the reverse effect; so that suppose sulphate of magnesia and muriate of soda to be submitted to the action of this diluted alcohol, aided by heat, the solvent power considered, in relation to the reverse combinations, may cause the change in the state of these compounds, and their transition into muriate of magnesia and sulphate of soda.

In the analysis of sea-water, then, by the first of the methods above described, the evaporation may either, if sulphate of magnesia and muriate of soda are the original ingredients, afford them undecomposed in the solid state; or, if muriate of magnesia and sulphate of soda are the ingredients, it may cause, by the influence of the force of cohesion, the formation of sulphate of magnesia and muriate of soda. But when the solid

solid mass is submitted to the action of alcohol, its operation, as a solvent, may, on the same principle, cause the reverse combinations to take place, of muriate of magnesia and sulphate of soda; the quantity of this sulphate being, of course, equivalent to the quantity of sulphate of magnesia; and the quantity of muriate of magnesia formed, being added to the quantity of that salt which the sea-water contains as a primary ingredient. Thus is explained the diversity of results obtained by the two modes of analysis; and this diversity itself affords an excellent illustration of the change of combination which may be produced in mineral waters by analytic operations, and a very conclusive proof that the substances obtained by the analysis are not always to be regarded as the original ingredients, since here they are varied according to the mode in which the analysis is performed *.

 LAVOISIER

* The small portion of sulphate of soda obtained with the sulphate of magnesia, in the second analysis, may have been formed by the action of the alcohol, which, though employed much less extensively than in the first, was still introduced to a certain extent. Or it might originate from other circumstances independent of this; for a similar result, I have been informed, sometimes occurs in the large way, sulphate of soda being procured in boiling down the bittern of sea-water to obtain its sulphate of magnesia, or in purifying this sulphate. The circumstances on which this depends, it may be difficult to assign with perfect precision; but it probably arises from the relative quantities of the different salts, and their tendency to crystallization, as influenced by the state of concentration, and the temperature. That both of these have a considerable effect on the combinations established in a compound saline solution, has been sufficiently shewn by the experiments of BERTHOLLET and others. A striking proof of it was derived from the very salts which are the subject of the present observations, in a singular case of affinity, first observed by SCHEELE, and afterwards confirmed by GREN: that of muriate of magnesia and sulphate of soda, which decompose each other in a concentrated solution at a high temperature, producing muriate of soda and sulphate of magnesia; but, at temperatures below 32°, the reverse effect takes place, muriate of soda and sulphate of

Lavoisier had stated muriate of lime as having been obtained in his analysis, being dissolved with the muriate of magnesia in the alcohol with which the solid matter obtained by evaporation had been lixiviated. I found no trace of it; and its presence after the evaporation to dryness, does not seem compatible with that of either sulphate of soda, or of magnesia. Yet if the preceding reasoning be just, it is possible that alcohol, by its solvent action, might cause its reproduction to a certain extent from sulphate of lime. On the other hand, the entire insolubility of sulphate of lime in alcohol, might prevent it from being acted on; this is even more probable; and the result stated of muriate of lime being obtained, is therefore, in all probability, to be ascribed to error, principally perhaps to its not being distinguished sufficiently from muriate of magnesia, the quantity of which is stated by Lavoisier evidently too low.

The

of magnesia re-acting, and being converted into sulphate of soda and muriate of magnesia. This singular case is evidently owing to the relation of the solubility of these salts to temperature. Muriate of soda has its solubility little increased by heat, of course little diminished by cold; sulphate of soda is in this respect precisely the reverse; hence, at an elevated temperature, muriate of soda is the less soluble salt; and this determines its formation and separation from a compound solution, containing its elements; at a low temperature, again, sulphate of soda is the less soluble salt; and this equally determines its formation, of course occasions the reverse decompositions. Now, according to the proportion of saline ingredients, and according to the state of concentration, and the temperature favouring the tendency of certain salts to crystallization more than others; it is easy to conceive, that in a compound solution, different combinations may be established, as these circumstances vary, and thus products may be obtained, under certain conditions, which are not obtained under others. Although sulphate of magnesia, therefore, is usually obtained by evaporation from sea-water, sulphate of soda, at some stages of the operation or under peculiar circumstances, with regard either to relative quantity of the elements, or to temperature, may likewise be formed.

The question now remains for consideration, What is the real composition of sea-water? How far are the salts obtained in either mode of analysis, those which exist originally in solution? This question is evidently to be considered under the same point of view as that which I have illustrated in a former paper, with regard to the change in the state of combination, which may be produced in the saline compounds existing in mineral waters, by the analytic operations to which they are subjected. We have no strict evidence that the binary compounds which are obtained are those which existed in solution, admitting even the principle that binary combinations exist. On the contrary, there is every probability, that the substances obtained are often products of the operation, arising from changes of combination which it established. And this is even placed in a more striking point of view in the present case, as the ingredients obtained are actually different, when different methods of analysis are employed. It is, therefore, necessary to inquire farther what the real composition is.

With regard to the sulphate of lime, which is the first substance separated by the evaporation, the general views I have already stated, give every probability to the conclusion, that it is a product of the operation formed by the action of sulphate of soda or of magnesia during the evaporation, on muriate of lime; that this last salt, therefore, is an ingredient in sea-water; the proportion of it being the quantity equivalent to that of the sulphate of lime which the analysis affords.

The small portions of carbonates of magnesia and lime which are obtained in the evaporation, I have already stated, are evidently accidental products from the decomposition of muriate of magnesia and muriate or sulphate of lime.

The large quantity of muriate of soda leaves no doubt, if binary compounds at all exist in a state of solution, that it is the

 chief

chief ingredient in sea-water, though the quantity of it may be affected by the changes which occur from the actions of some of the other salts.

A similar conclusion is to be drawn with regard to the Muriate of Magnesia. Though the proportion of it may be affected by the changes which occur in the analytic operations, still, from the quantity in which it is obtained, a considerable part of it must originally exist.

The principal difficulty is with regard to the sulphate of magnesia, and the sulphate of soda. It has always been supposed, that sulphate of magnesia is an ingredient in sea-water, from its being procured by evaporation ; and it is possible that it may be so. But it is just as possible, *a priori*, that sulphate of soda may be the original ingredient, and that, during the evaporation, the mutual action between it and muriate of magnesia, is favoured by the concentration, whence portions of both are decomposed, and corresponding quantities of sulphate of magnesia and muriate of soda are formed. Nor is there any thing connected with the mere results themselves, which proves which of these views is just.

If the appeal be made to experiment, it is sufficiently established, that sulphate of magnesia may be formed by the action of sulphate of soda on muriate of magnesia. When these two salts are boiled together in solution, a double decomposition takes place at least partially, and portions of sulphate of magnesia and muriate of soda are formed. On the other hand, the reverse combinations may also, according to circumstances, be established. We have seen reason to conclude, that they are so from the agency of alcohol ; and even in an aqueous solution, when certain proportions of the salts are used, they appear, under some circumstances, particularly that of a low temperature, to take place to a certain extent. But still these

facts

facts only shew what decompositions may occur from evaporation or other processes; they do not prove what the actual state of combination is in the original solution.

It is obvious, that this is merely a case belonging to the more general question, What is the state of combination in a compound saline solution, and on what principle can it be determined what are the binary compounds that really exist in it?—a question of considerable importance, but one at the same time of very difficult determination.

When it is admitted that this cannot be inferred with certainty from the actual products of analysis, the next most obvious view is, that it may be inferred from a knowledge of the real forces of affinity, as, according to these, certain binary compounds must be formed; and as the state of the science does not afford any certain estimate of the strength of attraction, the problem, it may be concluded, is at present incapable of being solved.

This conclusion, however, is by no means certain. Attraction is so much modified in its operation by external forces, and combinations are so frequently established from the influence of these, that it is not clear that we should be able to determine what combinations would exist in cases similar to those connected with the present investigation, from a knowledge of the degrees of attraction, were we even in possession of it. It is rather, perhaps, from a knowledge of the influence of these external forces, that an approximation to the solution of the problem is to be attained; and an extension of the principle I have illustrated in the preceding part of this paper, it appears to me may throw some light on the question.

If the force of cohesion has so much power in modifying chemical attraction, as to change its results, and establish combinations independent of the relative degrees of strength with which

which it is exerted; and if the reverse of cohesion, that is, the power of a solvent, operates in establishing the reverse combinations, as, in considering the agency of alcohol in this analysis, there has appeared sufficient reason to conclude, then it will follow, that, as in a concentrated medium, the least soluble compounds are formed, so in a dilute medium, the more soluble compounds will be established. The power of the solvent is exerted with greatest effect on those which are most soluble; and hence, if the reverse combinations even existed, this power must change them, and establish the others, precisely as the power of cohesion acts with most energy on those which are least soluble, and thus causes their formation, when it is brought to act with sufficient force. Hence will follow the simple rule by which the state of combination may be determined; that, in any fluid containing the elements of compound salts, the binary compounds existing in it will be those which are most soluble in that fluid; and the reverse combinations will only be established by its concentration favouring the influence of cohesion. Thus, if we concentrate a solution containing sulphuric and muriatic acids, soda and lime, we know, that from the influence of cohesion, the binary combinations will be those of sulphuric acid with lime, and of muriatic acid with soda. And on the same principle, we may infer, that in a dilute solution containing these elements, the combinations will, from the influence of the power the reverse of cohesion, that of the solvent action of the liquid, be those of sulphuric acid with soda, and muriatic acid with lime. In a concentrated solution, containing muriatic and sulphuric acids, soda and magnesia, sulphate of magnesia and muriate of soda are formed; and, on the same principle, in a dilute solution, there must exist sulphate of soda and muriate of magnesia.

This

This principle, if just, is an important one, as enabling us to determine the state of binary combinations in a saline liquor. I add, therefore, one other illustration of the reasoning on which it rests.

Suppose that in a compound saline solution, that is, one containing more than one acid and one base, the acid and the base which have the strongest attraction, are those which are most soluble, or form the most soluble compound; the solvent power of the liquid operating at the same time, will concur with this, and favour their combination; and any other acid and base likewise present, will of course, at the same time, combine. But suppose the more powerful attraction to belong to those which form an insoluble compound, the solvent power counteracts this, and prevents the combination. And the more this power is increased, which is done by increasing the quantity of the solvent, the more will this be counteracted. The reverse combinations will therefore be established by the operation of the opposite affinities. Hence, generally speaking, in a dilute solution, the binary combinations must be those which form the most soluble compounds, and very powerful attractions would be required to counteract this.

Applying this principle to the composition of sea-water, or rather to the question with regard to the sulphate of soda, and sulphate of magnesia, it is obvious, that the former is to be considered as the original ingredient, and the latter as a product of the evaporation; for muriate of magnesia, and sulphate of soda, are, on the whole, more soluble in water than muriate of soda and sulphate of magnesia. On the same principle it follows, still more unequivocally, that the lime exists in the state of muriate of lime, with a portion of sulphate of soda equivalent to the quantity of sulphate of lime which the evaporation affords. The salts, therefore, really existing in sea-water,

water, are muriate of soda, muriate of magnesia, muriate of lime, and sulphate of soda. The quantity of muriate of soda is less than what is obtained by evaporation, for a portion of it is formed by the decompositions which occur; the quantity of muriate of magnesia is larger, as a portion of it is decomposed; the quantity of muriate of lime is inferred from the quantity of sulphate of lime; and the quantity of sulphate of soda is determined from the quantities of sulphate of magnesia and sulphate of lime obtained. The proportions may thus be easily assigned. Referring to the preceding analyses, the proportions in a pint, according to this principle, will be the following:

According to the first analysis,.

Muriate of Soda, -	170.2 grains.
——— Magnesia,	30.6
——— Lime, -	5.8
Sulphate of Soda, -	21.9
	228.5 grains.

According to the second analysis:

Muriate of Soda, -	165.2
——— Magnesia,	31.6
——— Lime, -	5.9
Sulphate of Soda, -	24.9
	227.6

If the opposite view be adopted, that the sulphate existing in sea-water is not sulphate of soda, but sulphate of magnesia; then the ingredients and their proportions will be as follow:

According

According to the first analysis,

Muriate of Soda,	-	188.3 grains.
——— Magnesia,		16
——— Lime,	-	5.8
Sulphate of Magnesia,		18.4
		228.5

According to the second analysis,

Muriate of Soda,	-	185.6
——— Magnesia,		15.2
——— Lime,	-	5.9
Sulphate of Magnesia,		20.9
		227.6

But this view rests on no principle, and is, as I have stated, less probable than the other *.

 The

*There is sometimes obtained in the large way, from the products of the evaporation of sea-water, a triple salt, which has not been noticed by chemists, but which appears to be of definite composition, and is distinguished by peculiar properties,—a Sulphate of Magnesia and Soda. It is formed in purifying the sulphate of magnesia procured by the first evaporation from the bittern of sea-salt. In this process the sulphate, which is impure, both from the intermixture of muriate of soda and muriate of magnesia, and perhaps, also, of sulphate of soda, is dissolved in water, and by evaporation and cooling is obtained crystallised; a fresh quantity of it is added to the residual liquor, and by the necessary evaporation and cooling, a new crystallization is produced; this is repeated for a third or fourth time; and it is in these latter crystallizations that this triple salt is formed, frequently in considerable quantity, and usually at a high temperature, being precipitated even in the boiler. It crystallises in rhombs, at first irregular and semitransparent; but by solution in water, and a second crystallization, is obtained in more regular rhombs, truncated on the acute angles, on the obtuse angles and edges, and on the terminal edges,

The difficulties attending the perfect separation of compound salts from each other, by crystallization, even with the aid

edges, considering the rhomb as a four-sided prism, and transparent. The crystals are permanent in the air; they are soluble in little more than three times their weight of water, at the temperature of 60°; they do not undergo the watery fusion from heat, but suffer decrepitation. In these properties, this salt differs entirely from sulphate of soda, or sulphate of magnesia.

To determine its composition, 20 grains reduced to powder were exposed to heat, raised gradually nearly to redness; they lost from the escape of water 5.6 grains. The residual powder was dissolved in water, and muriate of barytes was added as long as any precipitation was produced. The precipitate dried at a red heat, weighed 23.9 grains, equivalent to sulphuric acid 8.2 grains. To the clear liquor carbonate of ammonia was added, which did not impair the transparency; phosphoric acid was then dropped in, which produced a copious precipitation. The precipitate, calcined at a red-heat, weighed 5.3 grains, equivalent to 2.1 of magnesia, or 6.4 of sulphate of magnesia; the residual liquor being evaporated to dryness, the dry mass was submitted to heat, gradually raised, as long as any vapours exhaled; it afforded, by solution in water and evaporation, muriate of soda in cubes, which, after exposure to a red heat, weighed 6.4 grains, equivalent to 7.8 grains of sulphate of soda. 100 grains of the salt, therefore, afford of

Sulphate of Magnesia, - -	32 grains.
Soda, -	39
Water of Crystallization, -	28
Loss, - -	1
	100

It afforded also a slight trace of muriatic acid; its solution being in a very slight degree rendered turbid by nitrate of silver, probably owing to the intermixture of a little muriate of soda, as an extraneous ingredient. This accounts for the proportion of sulphuric acid, as inferred from the quantities of the bases, being a little larger than that directly obtained by the precipitation by muriate of barytes.

The difference of crystalline form, as well as other differences of properties in the salt from those, either of sulphate of soda or sulphate of magnesia, sufficiently prove that it is not merely an intermixture of the two, but that it is of definite composition. It deserves to be remarked, too, that it has not the same relation to water that either of these salts has, or any mean between

aid of the action of alcohol, either as a solvent or a precipitant, are so great, that analyses executed in this mode can scarcely be perfectly accurate. And as it appears, if the preceding observations are just, that there is no certainty in the conclusion that the products of analysis by evaporation or crystallization are the real ingredients, no peculiar advantage in this respect belongs to this method, and just as much information is obtained by discovering the acids and bases which exist in solution, and then inferring, according to the most probable view, what the states of binary combination are in which they exist. This kind of analysis has the advantage, that it can be executed with much more precision than the other: it is liable to fewer sources of error, and, by finding the quantities, not of the compounds, but of the elements, any error that is introduced is discovered, when the binary compounds are inferred. To ensure accuracy, therefore, it was desirable to apply it to the illustration of the present subject, more especially as the preceding analyses, though they do not differ greatly in the results, still, from these difficulties, do not exactly correspond.

Different methods might be employed. The following is the one I have preferred.

To a pint of sea-water, reduced by evaporation to nearly one-fourth, at which state of concentration no crystallization nor deposition takes place, muriate of barytes was added as long as any precipitation occurred. By a preliminary trial, it

 was

tween them; the quantity of its water of crystallization being considerably less. Its taste is much less disagreeable than that of sulphate of soda or sulphate of magnesia; it might therefore probably be introduced with advantage as a purgative salt, especially as it could be procured at a low price; and from its composition, it would afford a very good substitute for the aperient mineral waters, which usually owe their activity to sulphate of soda and sulphate of magnesia.

was found that the precipitate gives no effervescence with muriatic acid, nor suffers any change. It was therefore sulphate of barytes. Dried by a low red heat, it weighed 43 grains, equivalent to 14.4 *sulphuric acid.*

By this step the whole salts in the sea-water were converted into muriates. It remained to discover and estimate the quantities of their bases.

To the clear liquor, oxalate of ammonia was added as long as any turbid appearance was produced. The precipitate, washed and dried, by a heat of 150° continued for two hours, weighed 8.5 grains. Calcined with a low red-heat, it gave of carbonate of lime 5.2 grains. This, dissolved with strong effervescence in dilute muriatic acid, and the product being heated with sulphuric acid, gave sulphate of lime, which, after exposure to a red-heat, weighed 7 grains, equivalent to 2.9 of pure *lime.*

To the clear liquor warmed, carbonate of ammonia was added, and phosphoric acid was dropped in *; an abundant precipitation took place of phosphate of magnesia and ammonia, and additional portions of the phosphoric acid, with such additions of the carbonate as were necessary to preserve an excess of ammonia in the liquor, were added, as long as any precipitation was produced. The precipitate was converted, by calcination for an hour at a red-heat, into phosphate of magnesia. This weighed 37 grains, equivalent to 14.8 grains of *magnesia.*

The clear liquor was evaporated to dryness, and the dry mass was exposed to a heat gradually raised to redness, to expel the muriate of ammonia formed in the preceding operations.

* I shall have to state in a subsequent paper, the peculiar advantages attending this method of estimating the magnesia.

tions. Muriate of soda remained, which weighed 180.5 grains, equivalent to 96.3 of soda, and 84.2 of muriatic acid.

This gives the quantity of *Soda* contained in the sea-water; but it does not necessarily give the quantity of muriatic acid; for if more of this acid be present than what the soda can neutralize, combined with portions of any of the other bases, (and from the former analysis this appears to be the case,) this quantity will be combined with ammonia in the preceding steps of the analysis, and is of course dissipated in the state of muriate of ammonia.

This will appear, and the quantity be discovered by ascertaining what proportion of these bases the quantity of sulphuric acid obtained by the analysis is capable of neutralizing, thus finding if any excess of them remain; and, from the quantity of this, discovering the quantity of muriatic acid, which would be requisite for saturation, which of course is the quantity lost. 2.9 of lime, the quantity of this earth obtained by the analysis, neutralize 4.1 of sulphuric acid; this deducted from 14.4, the quantity obtained, leaves 10.3, to neutralize which, 5.1 of magnesia are required; this deducted from 14.8, the quantity of magnesia, leaves 9.7 of that earth; to neutralize this, 13.5 of muriatic acid are required; and this added to the 84.2 in the muriate of soda, gives 97.7 grains as the total quantity of *Muriatic Acid.*

The elements, then, of the salts, in a pint of sea-water, are, by this analysis,

Lime, -	2.9 grains.
Magnesia, -	14.8
Soda, -	96.3
Sulphuric Acid,	14.4
Muriatic Acid,	97.7
	226.1 grains.

The

The proportions of the compound salts may be assigned from these, according to whatever view may appear most probable, of the state of combination in which they exist in sea-water, and thus the results may be compared with those of the former analyses.

Thus, supposing the elements to be combined in the modes in which they are obtained by evaporation, that is, as muriate of soda, muriate of magnesia, sulphate of magnesia, and sulphate of lime; the proportions of these salts in a pint, will be

Muriate of Soda,	-	180.5	grains.
——— Magnesia,	-	23	
Sulphate of Magnesia,	-	15.5	
——— Lime,	-	7.1	
		226.1	

Supposing that the lime exists as muriate of lime, (which is the most probable conclusion with regard to it), and farther supposing, that the sulphuric acid exists in the state of sulphate of magnesia, the proportions will be

Muriate of Soda,	-	180.5	grains.
——— Magnesia,	-	18.3	
——— Lime,	-	5.7	
Sulphate of Magnesia,	-	21.6	
		226.1	grains.

Or, lastly, supposing that the sulphuric acid exists in the state of sulphate of soda, the proportions will be

Muriate

Muriate of Soda, -	159.3	grains.
——— Magnesia, -	35.5	
——— Lime, -	5.7	
Sulphate of Soda, -	25.6	
	226.1	grains.

These proportions differ somewhat, though not very materially, from those found by the other modes of analysis. The principal differences consist in the quantity of magnesia, and of sulphuric acid being rather larger. This is evidently to be ascribed to the modes of detecting sulphuric acid by barytes, and magnesia by phosphoric acid and ammonia, being so perfect, that the entire quantities of them are found; while, in the other modes, from the difficulty of effecting the entire separation of salts from each other, a small portion of sulphate of magnesia, or of muriate of magnesia and sulphate of soda, had remained with the muriate of soda, and though subcarbonate of soda was employed to decompose them, this decomposition is not altogether perfect. In the mode of analysis, too, by re-agents, the presence of water in the products can be more completely excluded, and to this, probably, is to be ascribed the absolute quantity of saline matter being a little less according to this analysis, than it is in the others *.

Of

* In another analysis of sea-water, in which subcarbonate of ammonia was employed to precipitate the magnesia, a solution of it being added to the water concentrated by evaporation, the clear liquor, after the subsidence of the precipitate being evaporated to dryness, the saline matter being exposed to heat, to dissipate the muriate of ammonia; being re-dissolved in water, the subcarbonate of ammonia again added, and this repeated for a third, and even a fourth time, the results gave the following proportions of the elements,

Lime,

Of the different views which may be taken of the state of combination of the elements, I have already inferred, that the one which supposes the sulphuric acid to exist in the state of sulphate of soda, is the most probable; and as the mode of analysis by re-agents is the most accurate, the last table may be considered as that which exhibits the nighest approximation to the real composition of sea-water, both with regard to its ingredients, and their proportions.

I had proposed to add a few observations on the analysis of salt brines; but as they are merely applications of the principles already illustrated, it is sufficient to state them briefly, or to notice those which present rather striking results.

Klaproth has given a laborious investigation of the nature of these brines; in the greater number of them, he states as ingredients, muriate of soda, muriate of magnesia, muriate of lime, and sulphate of lime. It is obvious, that there are no just grounds whence this composition can be inferred; it is much more probable, that sulphate of soda is the ingredient, and

Lime - -	2.9 grains.
Magnesia, - -	13.
Soda, - -	97.6
Sulphuric Acid, -	15.2
Muriatic Acid, -	96.9
	225.6 grains.

The principal difference here, is the proportion of magnesia being somewhat smaller, evidently owing to its precipitation by the carbonate of ammonia, even with the aid of the methods employed to promote it, being imperfect.

and that, acting on a portion of the muriate of lime, it forms sulphate of lime.

In other analyses, in those, for example, of the salt brines of Lorraine, by NICHOLAS *, muriate of soda, sulphate of soda, sulphate of lime, muriate of magnesia, and muriate of lime, are enumerated as ingredients. Here it is still more evident, that there is no proof of the previous existence of sulphate of lime; on the contrary, as both muriate of lime and sulphate of soda are present, they must, in the concentration of the liquor by evaporation, form by their mutual action muriate of soda and sulphate of lime; and the quantity therefore of this sulphate which may be obtained, must have this origin.

In general, when muriate of lime and muriate of magnesia are stated as ingredients with sulphate of lime, no sulphate of soda or sulphate of magnesia is found. The reason is obvious, that if either of these salts existed, it would re-act on the muriate of lime, and form sulphate of lime. But when muriate of lime is not found, sulphate of magnesia, or sulphate of soda, is often stated as an ingredient, obviously owing to the circumstance, that although a portion of muriate of lime has been present, so as to form sulphate of lime, there has not been a quantity sufficient to decompose the whole sulphate of soda, or sulphate of magnesia.

A striking example of these facts is to be found in Dr HENRY's analysis of the different varieties of sea and rock salt †. In four varieties of rock-salt, there were found small quantities of muriates of lime and magnesia, with a portion of sulphate of lime, but no sulphate of magnesia; while in the different varieties of sea-salt, British and foreign, there was no appreciable quantity, and in some of them no trace whatever

 of

* *Annales de Chimie*, t. 20.

† *Philosophical Transactions*, 1810.

of muriate of lime, but in all of them with sulphate of lime considerable quantities of sulphate of magnesia. In the latter, therefore, the muriate of lime had been converted entirely into sulphate of lime from the excess of sulphate of magnesia; in the former, from the deficiency of the sulphate, a portion of the muriate of lime had remained undecomposed.

A result somewhat similar, and which affords a very direct application of the same principles, is stated by Mr Horner, in his analysis of the salt-brine at Droitwich, compared with that of Cheshire *. The latter contains a little muriate of lime; the former contains none. But, then, that of Droitwich contains sulphate of soda and sulphate of lime; there is every probability, therefore, that its muriate of lime has been converted into sulphate of lime by the sulphate of soda, which is in excess; while in the Cheshire brine, as there is no sulphate of soda in excess, that is, none after the evaporation, a portion of muriate of lime remains.

There is a singular fact stated by Dr Henry with regard to what is called *fishery salt*, prepared from salt brine, which seems to admit of explanation only on these views. He found the proportion of sulphate of lime mixed with it to be less, as it was collected at a later period of the evaporation; that drawn from the boiler, after two hours application of the heat, contained in 100 parts 16 of sulphate of lime; that, after 4 hours, contained only 11; and that, after 6 hours, only $3\frac{1}{2}$. Now if the water of this brine held sulphate of lime in solution, the sulphate would begin to be deposited when the quantity of water was diminished to that extent that it was unable to retain the whole dissolved; and, in the progress of the evaporation, would continue to be deposited proportional to this, to

* *Geological Transactions*, vol. ii.

to the end of it, and the muriate of soda would be deposited in the same manner; so that the proportion between the two would continue nearly the same. But if the sulphate of lime did not exist in solution, but derives its origin from the action of sulphate of soda on muriate of lime, which these brines contain, this action would take place, when a certain degree of concentration of the liquor had been attained; the sulphate of lime would then be copiously deposited; but as the evaporation continued to proceed, its quantity would be diminished, as the quantity either of sulphate of soda, or of muriate of lime, became less; and its deposition would cease when either of these salts was exhausted.

This is placed in a still clearer light, by an analysis of these brines, after evaporation, to a certain extent, compared with their original composition. A brine from Northwich was found by Dr Henry to afford, by evaporation, saline matter, which, he inferred, contained in 1000 parts, muriate of lime and muriate of magnesia in nearly equal proportions 5 parts, sulphate of lime 19 parts, muriate of soda 974 parts. But the brine remaining after the separation of all the common salt, which it is thought worth while to extract, afforded saline matter by evaporation, which he found to contain, in 1000 parts, muriate of magnesia 35, muriate of lime 32, sulphate of lime 6, muriate of soda 927. Here, in the progress of the evaporation, the quantity of sulphate of soda, which may be considered as an original ingredient of the brine, had been diminished by the decomposition arising from its action on the muriate of lime. The liquor, therefore, after this, afforded by farther evaporation, along with a large quantity of muriate of lime, a small quantity only of sulphate of lime; while, if this sulphate had been an original ingredient, it would have continued to be afforded at least in equal proportion.

Something similar to this occurs in the evaporation of sea-water. It is, after a certain extent of evaporation, but while a large portion of liquor still remains, that the precipitation of sulphate of lime takes place; that is, after the concentration is sufficient to favour the mutual action of the sulphate of soda, or sulphate of magnesia, and muriate of lime. After this, the quantity diminishes as the evaporation proceeds, till at last not a trace of it, or of sulphate of lime, remains in the bittern, which consists of muriate of soda, muriate of magnesia, and sulphate of magnesia alone. This curious fact has not been particularly noticed, though it is in consequence of it that magnesia is prepared from bittern on the large scale, perfectly pure.

All these facts seem scarcely to admit of any explanation, but on the view that has been stated, and they afford a strong confirmation of it.

XII.

XII. *Elementary Demonstration of the Composition of Pressures.* *By* THOMAS JACKSON, LL. D. F. R. S. EDIN. and Professor of Natural Philosophy in the University of St. Andrew's.

(Read June 3. 1816.)

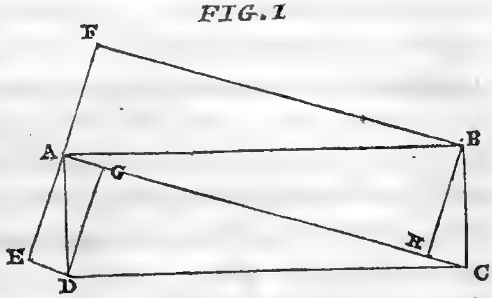
IT is well-known as a fundamental principle in statics, that "Two pressures, represented in direction and quantity by "two adjoining sides of a parallelogram, are equivalent to one "represented in direction and quantity by the diagonal "which passes through the point at which these sides meet." A demonstration of this proposition, that shall be at once sufficiently concise, and sufficiently elementary, to admit of its being with propriety introduced into a course of academical instruction, has been hitherto, so far as I know, a *desideratum*. The following may perhaps be found to possess that advantage.

LEMMA.

"If the equivalent of two pressures, represented by the adjoining sides of a rectangle, given in species, be always represented in direction by the corresponding diagonal, it shall "be represented by the same in quantity."

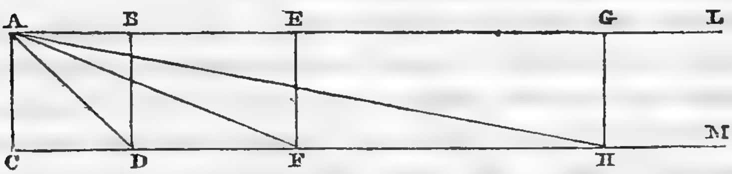
Let

FIG. 1



Let ABCD be a rectangle, and AC the diagonal passing through A; draw EAF perpendicular to AC, and let fall the perpendiculars BF, BH, DG, DE: then shall ABCD, AFBH and AEDG be similar rectangles; and if, in each of these, the equivalent of the pressures represented in direction and quantity by the sides, be represented in direction by the diagonal, it shall be represented by the same in quantity also. For if the forces AH and AF be equivalent to m AB; AE and AG shall be equivalent to m AD; and AB and AD to m AC; or m AB and m AD to m^2AC: that is, the forces AH, AF, AE, and AG, will be equivalent to m^2 AC: But AE and AF are equal and opposite: hence the forces AH and AG are equivalent to m^2 AC. But AH and AG are equivalent to AC; therefore $m = 1$.

FIG. 2



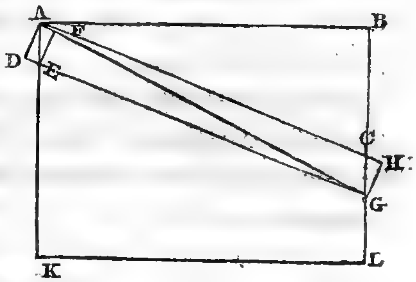
1. Now let ABDC be any square; and let the sides AB and CD be produced indefinitely towards B and D; draw the diagonal AD; in CD produced, take DF equal to AD; join AF; take FH equal to AF; join AH, and so on. And complete the rectangles ACFE, ACHG, &c.

It is obvious, that AD, AF, AH, &c. bisect the angles BAC, BAD, BAF, &c. respectively. Hence the resultant of AB

AB and AC, which are equal, must be represented in direction by AD; and therefore by the same in quantity also. The equivalent of AE and AC being the same with that of BE and AD, which are equal, will be represented in direction, and therefore in quantity by AF, (*vid.* Lemma). Thus may the proposition be proved of any rectangle whose diagonal makes with one of the sides any angle found by the continued bisection of a right angle.

2. Let (a) be any angle in the series above mentioned; the proposition shall be true in relation to any rectangle whose diagonal forms with one of the sides an angle that is any multiple of (a).

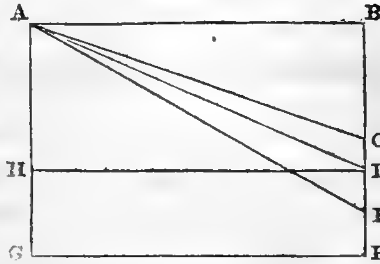
FIG. 3



Let AB and BC be two sides of a rectangle whose diagonal AC makes with AB an angle in relation to which the proposition has been already proved; and let CAG be equal to (a); the proposition shall be true in relation to the angle BAG; for let GED be parallel to AC, and draw the perpendiculars AD, EF, GH. It is already proved, that two forces represented by AD and AF are equivalent to the single force represented by AE; for $\angle$ DAE = $\angle$ BAC. AE may therefore be resolved into AD and AF; that is, the forces AE and AC are equivalent to the forces AD and AH, or to the single force AG. Since, then, AB and BC are equivalent to AC; and AC and CG equivalent to AG; AB and BG must be equivalent to AG.

3. Let

FIG. 4



3. Let BAD be an angle incommensurable with a right angle. The proposition is true in relation to BAC, the multiple of (a) next less than BAD, and of BAE, the multiple of (a) next higher, the difference between which ($= a$) may be less than any assigned angle. But the equivalent of AB and BD must, in respect of direction, be always intermediate between the equivalent of AB, BC, and that of AB, BE. It must, therefore, pass through D; and this is evidently true in the case of any similar rectangle; that is, so long as the angle BAD remains the same. Hence it must be represented by AD, also in quantity (*vid.* Lemma).

From what is said above in § 2. it is manifest that what has now been proved universally of the rectangle, may be extended to the oblique-angled parallelogram.

XIII.

XIII. *Account of the remarkable Case of* Margaret Lyall, *who continued in a State of Sleep nearly Six Weeks. By the* Reverend James Brewster, *Minister of Craig. Communicated by* Dr Brewster.

(Read February 19. 1816.*)*

My Dear Brother, *Manse of Craig, Feb.* 16. 1816.

THE inclosed account was drawn up at the request of Robert Græme, Esq. when all the circumstances were fresh in my own recollection, and that of all with whom I had occasion to confer on the subject. Since you requested me to send you a correct copy of the whole case, I have renewed my inquiries among the friends of the young woman, and submitted my account to several persons, who were most capable of supplying any omissions, or correcting any mistakes. I can confidently vouch for the general accuracy of the statement; but would not wish its credibility to rest entirely on my single testimony. I have, therefore, procured the signature of the young woman's father, and of several gentlemen, with whom you are more or less acquainted, and who frequently saw her during her illness. The account of her recovery, on the 8th of August, indeed, rests wholly on the testimony of the father, which there is not the smallest reason to doubt. I am sensible, that many of the circumstances which I have mentioned, may appear to be unnecessarily

cessarily minute, or even altogether unimportant; but, in detailing so remarkable a case, I did not think myself qualified or entitled to select according to my own judgment; and considered it to be my business, as a reporter, merely to relate, as clearly and correctly as possible, whatever was observable in the situation of the patient. I have noted, also, her previous employment, the places where she resided, and some of the individuals who attended to her case, partly to render the account more intelligible, and partly to enable others to make farther inquiries for themselves. I may mention farther, in case you may not be aware of the circumstance, that there is a similar case recorded in the *Transactions of the Royal Society of London* for 1705, vol. xxiv. p. 2177.

Yours, &c.

To Dr Brewster. Jas. Brewster.

Margaret Lyall, a young woman, about twenty-one years of age, daughter of John Lyall, shoemaker in the parish of Marytown, served during the winter half-year preceding Whitsunday 1815, in the family of Peter Arkley, Esq. of Dunninald, in the parish of Craig. At the last mentioned term, she went as servant to the Reverend Mr Foote of Logie; but, in a few days after entering her place, was seized with a slow fever, which confined her to bed rather more than a fortnight. During the latter part of her illness, she was conveyed to her father's house; and, on the 23d of June, about eight days after she had been able to leave her bed, she resumed her situation with Mrs Foote, who had, in the mean time, removed to Budden, in the parish of Craig, for the benefit of sea-bathing. She was observed, after her return, to do her work rather in a hurried manner; and, when sent upon any errand, to run

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or walk very quickly, as if impatient to finish whatever she had in hand. Her health, however, appeared to be perfectly restored, except that her menses were obstructed. On Tuesday morning, June 27th, about four days after her return to service, she was found in bed in a deep sleep, with the appearance of blood having flowed from her nose; and about half a Scotch pint of blood was perceived on the floor, at her bed-side. All attempts to awaken her were utterly ineffectual; and she was conveyed in a cart to her father's house, about half a mile distant from Budden. Dr Gibson, physician in Montrose, having been called, a pound of blood was taken from her arm; but she still remained in the same lethargic state, without making the slightest motion, or taking any nourishment, or having any kind of evacuation, till the afternoon of Friday the 30th day of June, when she awoke of her own accord, and asked for food. At this period she possessed all her mental and bodily faculties; mentioned distinctly, that she recollected her having been awakened on Tuesday morning at two o'clock, by a bleeding at her nose, which flowed very rapidly; said, that she held her head over the bed-side till the bleeding stopped; but declared, that, from that moment, she had no feeling or remembrance of any thing, and felt only as if she had taken a very long sleep. An injection was administered with good effect, and she went to sleep as usual; but, next morning, (Saturday, July 1.), she was found in the same state of profound sleep as before. Her breathing was so gentle as to be scarcely perceptible; her countenance remarkably placid, and free from any expression of distress; but her jaws were so firmly locked, that no kind of food or liquid could be introduced into her mouth. In this situation she continued for the space of seven days, without any motion, food, or evacuation either of urine or fæces. At the end of seven days she began to move her left hand; and, by

pointing it to her mouth, signified a wish for food. She took readily whatever was given to her, and shewed an inclination to eat more than was thought advisable by the medical attendants. Still, however, she discovered no symptoms of hearing, and made no other kind of bodily movement, than that of her left hand. Her right hand and arm, particularly, appeared completely dead and devoid of feeling, and, even when pricked with a pin, so as to draw blood, never shrunk in the smallest degree, or indicated the slightest sense of pain. At the same time, she instantly drew back the left arm, whenever it was touched by the point of the pin. She continued to take food, whenever it was offered to her; and when the bread was put into her left hand, and the hand raised by another person to her mouth, she immediately began to eat slowly, but unremittingly, munching like a rabbit, till it was finished. It was remarked, that, if it happened to be a slice of loaf which she was eating, she turned the crust, when she came to it, so as to introduce it more easily into her mouth, as if she had been fully sensible of what she was doing. But when she had ceased to eat, her hand dropped upon her chin or under lip, and rested there, till it was replaced by her side, or upon her breast. She took medicine, when it was administered, as readily as food, without any indication of disgust; and, in this way, by means of castor oil and aloetic pills, her bowels were kept open; but no evacuation ever took place without the use of a laxative. It was observed, that she always gave a signal, by pushing down the bed-clothes, when she had occasion to make any evacuation. The eye-lids were uniformly shut, and, when forced open, the ball of the eye appeared turned upwards, so as to shew only the white part of it. Her friends shewed considerable reluctance to allow any medical means to be used for her recovery; but, about the middle of July, her head was shaved, and a large blister

blister applied, which remained nineteen hours, and produced an abundant issue, yet without exciting the smallest symptom of uneasiness in the patient. Sinapisms were also applied to her feet, and her legs were moved from hot water into cold, and *vice versâ*, without any appearance of sensation. In this state she remained, without any apparent alteration, till Tuesday the 8th day of August, precisely six weeks from the time when she was first seized with her lethargy, and without ever appearing to be awake, except, as mentioned, on the afternoon of Friday the 30th of June. During the whole of this period, her colour was generally that of health; but her complexion rather more delicate than usual, and occasionally changing, sometimes to paleness, and at other times to a feverish flush. The heat of her body was natural; but, when lifted out of bed, she generally became remarkably cold. The state of her pulse was not regularly marked; but, during the first two weeks, it was generally at 50; during the third and fourth week, about 60; and, on the day before her recovery, at 70 or 72; whether its increase was gradual was not ascertained. She continued, during the whole period, to breathe in the same soft and almost imperceptible manner as at first; but was observed occasionally, during the night time, to draw her breath more strongly, like a person who had fallen asleep. She discovered no symptoms of hearing, till about four days before her recovery, when, upon being requested, (as she had often been before, without effect), to give a sign if she heard what was said to her, she made a slight motion with her left hand, but soon ceased again to shew any sense of hearing. On Tuesday forenoon, the day of her recovery, she shewed evident signs of hearing; and by moving her left hand, intimated her assent or dissent in a tolerably intelligent manner; yet, in the afternoon of the same day, she seemed to have again entirely lost all sense of hearing.

ing. About eight o'clock on Tuesday evening, her father, a shrewd intelligent man, and of a most respectable character, anxious to avail himself of her recovered sense of hearing, and hoping to rouse her faculties by alarming her fears *, sat down at her bed-side, and told her that he had now given consent, (as was in fact the case,) that she should be removed to the Montrose Infirmary; that, as her case was remarkable, the Doctors would naturally try every kind of experiment for her recovery; that he was very much distressed, by being obliged to put her entirely into their hands; and would "fain hope," that this measure might still be rendered unnecessary, by her getting better before the time fixed for her removal. She gave evident signs of hearing him, and assented to his proposal of having the usual family-worship in her bed-room. After this was over, she was lifted into a chair till her bed should be made; and her father, taking hold of her right hand, urged her to make an exertion to move it. She began to move first the thumb, then the rest of the fingers in succession, and next her toes in like manner. He then opened her eye-lids; and, presenting a candle, desired her to look at it, and asked, whether she saw it. She answered, "Yes," in a low and feeble voice. She now proceeded gradually, and in a very few minutes, to regain all her faculties; but was so weak as scarcely to be able to move. Upon being interrogated respecting her extraordinary

* Lest it might be supposed, that this procedure of the father implied a suspicion on his part of some deception being practised by the young woman, it may be proper to state, that it was suggested by his own experience in the case of another daughter, who had been affected many years before in a very extraordinary degree, with St Vitus's dance, or, as it is termed in this country, "The "louping ague;" and who was almost instantaneously cured by the application of terror.

nary state, she mentioned, that she had no knowledge of any thing that had happened; that she remembered, indeed, having conversed with her friends at her former-awakening, (Friday afternoon 30th of June), but felt it a great exertion then to speak to them; that she recollected also having heard the voice of Mr Cowie, Minister in Montrose, (the person who spoke to her on the forenoon of Tuesday the 8th of August,) but did not hear the persons who spoke to her on the afternoon of the same day; that she had never been conscious of having either needed or received food, of having been lifted to make evacuations, or of any other circumstance in her case. She had no idea of her having been blistered; and expressed great surprise, upon discovering that her head was shaved. She continued in a very feeble state for a few days, but took her food nearly as usual, and improved in strength so rapidly, that on the last day of August she began to work as a reaper in the service of Mr Arkley of Dunninald; and continued to perform the regular labour of the harvest for three weeks, without any inconvenience, except being extremely fatigued the first day.

After the conclusion of the harvest, she went into Mr Arkley's family, as a servant; and on the 27th day of September, was found in the morning, by her fellow-servants, in her former state of profound sleep, from which they were unable to rouse her. She was conveyed immediately to her father's house, (little more than a quarter of a mile distant,) and remained exactly fifty hours in a gentle, but deep sleep, without making any kind of evacuation, or taking any kind of nourishment. Upon awakening, she arose apparently in perfect health, took her breakfast, and resumed her work as usual at Dunninald. On the 11th of October, she was again found in the

the morning in the same lethargic state; was removed to the house of her father, where she awoke, as before, after the same period of fifty hours sleep; and returned to her service, without seeming to have experienced any inconvenience. At both of these times her menses were obstructed. Dr HENDERSON, physician in Dundee, who happened to be on a visit to his friends at Dunninald, prescribed some medicines suited to that complaint; and she has ever since been in good health, and able to continue in service *.

(Signed) JAS. BREWSTER,
Minister of Craig.

* On the morning of September 21. 1816, MARGARET LYALL, whose case is described above, was found in an out-house at Dunninald, hanged by her own hands. No cause could be assigned for this unhappy act. Her health had been good since the month of October 1815; and she had been comfortable in her situation. It was thought by the family, that, a day or two preceding her death, her eyes had the appearance of rolling rather wildly; but she had assisted the day before in serving the table, and been in good spirits that evening. On the following morning, she was seen to bring in the milk as usual, and was heard to say in passing rather hurriedly through a room, where the other maids were at work, that something had gone wrong about her dairy; but was not seen again till she was found dead about half an hour after. She is known to have had a strong abhorrence of the idea of her former distress recurring; and to have occasionally manifested, especially before her first long sleep, the greatest depression of spirits, and even disgust of life.

I hereby certify the preceding account of my daughter Margaret's illness and recovery to be correct in every circumstance, according to the best of my recollection.

(Signed) John Lyal.

We hereby attest, That the above-mentioned particulars in the extraordinary case of Margaret Lyall, are either consistent with our personal knowledge, or agreeable to all that we have heard from the most credible testimony.

(Signed) Peter Arkley of Dunninald.
Andw. Fergusson, Minister of Maryton.
Willm. Gibson, Physician in Montrose.

I hereby certify the preceding account of my daughter's case, illness and recovery, to be correct in every circumstance, to the best of my recollection.

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[illegible] cent with our personal knowledge, [illegible]

[illegible] (German Physician in Florence) [illegible]

[illegible] familiar with such instances, it presents the difficulty of [illegible]

[illegible] the following observations.

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XIV. *A General Formula for the Analysis of Mineral Waters.* *By* John Murray, M. D. F. R. S. E.

(Read June 7. 1816.)

THE analysis of Mineral Waters has always been considered as a difficult operation. Numerous methods are employed to discover their ingredients, and estimate their quantities, many of which are liable to errors. This diversity of method itself is a source of discordant results. And to those not familiar with such researches, it presents the difficulty often of determining what process is best adapted to discover a particular composition. Hence the advantage of a general formula, if this could be given, applicable to the analysis of all waters. The views which have been stated in the papers, connected with this subject, which I have had the honour of submitting to the Society, have suggested a method which appears to me to admit of very general application, and to be simple, not difficult of execution, nor liable to any sources of error but what may be easily obviated. The principles on which this method is founded, and the details of the process itself, form the subject of the following observations.

Two methods of analysis have been employed for discovering the composition of mineral waters,—what may be called

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the *direct method*, in which, by evaporation, aided by the subsequent application of solvents, or sometimes by precipitants, certain compound salts are obtained; and what may be called the *indirect method*, in which, by the use of re-agents, the principles of these salts, that is, the acids and bases of which they are formed are discovered, and their quantities estimated, whence the particular salts, and their proportions, may be inferred.

Chemists have always considered the former of these methods as affording the most certain and essential information: they have not neglected the latter; but they have usually employed it as subordinate to the other. The salts procured by evaporation, have been uniformly considered as the real ingredients, and nothing more was required, therefore, it was imagined for the accuracy of the analysis, than the obtaining them pure, and estimating their quantities with precision. On the contrary, in obtaining the elements merely, no information, it was believed, was gained with regard to the real composition, for it still remained to be determined, in what mode they were combined, and this, it was supposed, could be inferred only from the compounds actually obtained. This method, therefore, when employed with a view to estimate quantities, has been had recourse to only to obviate particular difficulties attending the execution of the other, or to give greater accuracy to the proportions, or, at farthest, when the composition is very simple, consisting chiefly of one genus of salts.

Another circumstance contributed to lead to a preference of the direct mode of analysis;—the uncertainty attending the determination of the proportions of the elements of compound salts. This uncertainty was such, that even from the most exact determination of the absolute quantities of the acids and bases existing in a mineral water, it would have been difficult, or

or nearly impracticable, to assign the precise composition, and the real proportions of the compound salts; and hence the necessity of employing the direct method of obtaining them.

The present state of the science leads to other views.

If the conclusion were just, that the salts obtained by evaporation, or any analogous process from a mineral water, are its real ingredients, no doubt could remain of the superiority of the direct method of analysis; and even of the absolute necessity of employing it. But no illustrations, I believe, are required to prove, that this conclusion is not necessarily true. The concentration by the evaporation, must, in many cases, change the state of combination, and the salts obtained are hence frequently products of the operation, not original ingredients. Whether they are so or not, and what the real composition is, are to be determined on other grounds than on their being actually obtained; and no more information is gained, therefore, with regard to that composition, by their being procured, than by their elements being discovered; for when these are known, and their quantities are determined, we can, according to the principle from which the actual modes of combination are inferred, whatever this may be, assign with equal facility the quantities of the binary compounds they form.

The accuracy with which the proportions of the constituent principles of the greater number of the compound salts are now determined, enables us also to do this with as much precision, as by obtaining the compounds themselves. And if any error should exist in the estimation of these proportions, the prosecution of these researches could not fail soon to discover it.

The mode of determining the composition of a mineral water, by discovering the acids and bases which it contains, admits, in general, of greater facility of execution, and more accuracy,

curacy, than the mode of determining it by obtaining insulated the compound salts. Nothing is more difficult than to effect the entire separation of salts by crystallization, aided even by the usual methods of the action of alcohol, either as a solvent or a precipitant, or by the action of water as a solvent at different temperatures; in many cases it cannot be completely attained, and the analysis must be deficient in accuracy. No such difficulty is attached to the other method. The principles being discovered, and their quantities estimated in general from their precipitation in insoluble compounds, their entire separation is easily effected. Nothing is easier, for example, than to estimate the total quantity of sulphuric acid by precipitation by barytes, or of lime by precipitation by oxalic acid. And this method has one peculiar advantage with regard to accuracy, that if any error is committed in the estimation of any of the principles, it is discovered in the subsequent step of inferring the binary combinations, since, if all the elements do not bear that due proportion to each other which is necessary to produce the state of neutralization, the excess or deficiency becomes apparent, and of course the error is detected. The indirect method, then, has every advantage over the other, both in accuracy and facility of execution.

Another advantage is derived from these views, if they are just, that of precluding the discussion of questions which otherwise fall to be considered, and which must often be of difficult determination, if they are even capable of being determined. From the state of combination being liable to be influenced by evaporation, or any other analytic operation by which the salts existing in a mineral water are attempted to be procured, discordant results will often be obtained, according to the methods employed; the proportions at least will be different, and sometimes even products will be found by one method which

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are not by another. In a water which is of complicated composition, this will more peculiarly be the case. The Cheltenham waters, for example, have, in different analyses, afforded results considerably different; and, on the supposition of the salts procured being the real ingredients, this diversity must be ascribed to inaccuracy, and ample room for discussion, with regard to this is introduced. In like manner, it has often been a subject of controversy, whether sea-water contains sulphate of soda with sulphate of magnesia. All such discussions, however, are superfluous. The salts procured are not necessarily the real ingredients, but in part, at least, are products of the operation, liable, therefore, to be obtained or not, or to be obtained in different proportions, according to the method employed. And all that can be done with precision, is to estimate the elements, and then to exhibit their binary combinations according to whatever may be the most probable view of the real composition.

The process I have to state, conformable to these views, is essentially the same as that which I employed in the analysis of sea-water in a preceding memoir; and it was the consideration of the advantages belonging to it, that has led me to propose it, with the necessary modifications, as one of general application.

Mineral waters have been arranged under the four classes of Carbonated, Sulphureous, Chalybeate, and Saline. But all of them are either saline, or may be reduced under this division. From waters of the first class, the carbonic acid which is in excess, is expelled by heat, and its quantity is estimated. Sulphuretted hydrogen is in like manner expelled or decomposed. And iron may be detected by its particular tests, and removed by appropriate methods. In all these cases the water remains, with any saline impregnation which it has, and of course is es-

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sentially the same in the subsequent steps of its analysis as a water purely saline; the precaution only being observed of these principles being removed, and of no new ingredient being introduced by the methods employed.

The salts usually contained in mineral waters are Carbonates, Sulphates, and Muriates, of Lime, of Magnesia and of Soda. In proceeding to the analysis, a general knowledge is of course first to be gained of the probable composition by the application of the usual tests; the presence of sulphuric and carbonic acids being detected by nitrate of barytes, of muriatic acid by nitrate of silver, of lime by oxalic acid, of magnesia by lime-water or ammonia, and of any alkaline neutral salt by evaporation. It will also be of advantage to obtain the products of evaporation, and ascertain their quantities, without any minute attention to precision, the object being merely, by these previous steps, to facilitate the more accurate analysis.

Supposing this to be done, and supposing the composition of the water to be of the most complicated kind, that is, that by the indications from tests, or by evaporation, it has afforded carbonates, sulphates, and muriates of lime, magnesia and soda, the following is the general process to be followed to ascertain the ingredients, and their proportions.

Reduce the water by evaporation, as far as can be done without occasioning any sensible precipitation or crystallization; this, by the concentration, rendering the operation of the re-agents to be employed more certain and complete. It also removes any free carbonic acid.

Add to the water thus concentrated a saturated solution of muriate of barytes, as long as any precipitation is produced, taking care to avoid adding an excess. By a previous experiment, let it be ascertained whether this precipitate effervesces or not with

with diluted muriatic acid, and whether it is entirely dissolved. If it is, the precipitate is of course carbonate of barytes, the weight of which, when it is dried, gives the quantity of carbonic acid; 100 grains containing 22 of acid. If it do not effervesce, it is sulphate of barytes, the weight of which, in like manner, gives the quantity of sulphuric acid; 100 grains, dried at a low red-heat, containing 34 of acid. If it effervesce, and is partially dissolved, it consists both of carbonate and sulphate. To ascertain the proportions of these, let the precipitate be dried at a heat a little inferior to redness, and weighed; then submit it to the action of dilute muriatic acid; after this wash it with water, and dry it by a similar heat, its weight will give the quantity of sulphate, and the loss of weight, that of carbonate of barytes.

By this operation the carbonic and sulphuric acids are entirely removed, and the whole salts in the water are converted into muriates. It remains, therefore, first to discover and estimate the quantities of the bases present, and then, to complete the analysis, to find the quantity of muriatic acid originally contained.

Add to the clear liquor a saturated solution of oxalate of ammonia as long as any turbid appearance is produced. The lime will be thrown down in the state of oxalate. The precipitate being washed, may be dried, but as it cannot be exposed to a red-heat without decomposition, it can scarcely be brought to any uniform state of dryness with sufficient accuracy to admit of the quantity of lime being estimated from its weight. It is therefore to be calcined with a low red-heat, by which it is converted into carbonate of lime, 100 grains of which are equivalent to 56 of lime. But as a portion of carbonic acid may be expelled, if the heat is raised too high, or a little water retained if it is not high enough; it is proper to

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convert it into sulphate, by adding sulphuric acid to a slight excess, and then exposing to a full red-heat. The dry sulphate of lime will remain, 100 grains of which contain 41.5 of lime *.

The next step is to precipitate the magnesia. With regard to this there is some difficulty, particularly as connected with the design of the present formula. The principle on which it is founded is, first, to remove all the acids but the muriatic, and, secondly, to remove the bases, or otherwise estimate their quantities. The lime and the magnesia may be removed by precipitation; the soda cannot. The process, therefore, must be so conducted, as to leave it at the end in the state of muriate of soda. Hence it is necessary either to remove any new product introduced in the previous steps of the analysis, or if any such remain, to be able to estimate its quantity with precision. In decomposing the muriate of lime by oxalate of ammonia, muriate of ammonia is substituted, which can be afterwards dissipated by heat. The object, therefore, is to decompose the muriate of magnesia, and remove the magnesia, either by some similar method, or, if not, by some other in which the muriate substituted can be accurately estimated; and to attain one or other of these conditions, gives rise to the difficulty to which I have alluded.

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* The only source of error to which this step of the analysis is liable, is that which will arise if more barytes has been used in the first operation, than was necessary to precipitate the sulphuric and carbonic acids. It will be thrown down in the state of oxalate of barytes, and be converted into carbonate and sulphate, and thus give the apparent proportion of lime too large. This is obviated, of course, by taking care to avoid using an excess of barytes. To render the operation of the oxalate of ammonia as perfect as possible in precipitating the lime, the water should be considerably reduced by evaporation, taking care to avoid any separation of any of its ingredients.

The decomposition of the magnesian salt by ammonia would have the former advantage, as the muriate of ammonia would be expelled at the end of the process by heat; but this decomposition, it is well known, is only partial. Sub-carbonate of ammonia causes a more abundant precipitation of magnesia, but still its action is likewise partial, a ternary soluble salt being formed after a certain quantity has been added. It seemed probable, that this might be obviated, by adding the sub-carbonate of ammonia as long as it occasioned any precipitation, then evaporating the clear liquor to dryness, expelling the muriate of ammonia, and any excess of ammonia, by heat, re-dissolving, and again adding the sub-carbonate of ammonia to decompose the remaining magnesian salt. Proceeding in this way, I found that a copious precipitation took place on the second addition, and even at the fourth a small quantity of precipitate was thrown down. But the decomposition, after all, was not perfect, for the quantity of magnesia obtained was not equal to what was procured by other methods.

Sub-carbonate of soda or potash has been usually employed to precipitate magnesia from its saline combinations. The precipitation, however, is only partial, unless an excess of the precipitant be employed (and even then, perhaps, is not altogether complete); and as this exces cannot easily be estimated, it introduces a source of error in estimating the quantity of muriate of soda at the end of the operation, against which it is not easy to guard.

The method proposed by Dr Wollaston, of precipitating magnesia from its solution, by first adding carbonate of ammonia, and then phosphate of soda, so as to form the insoluble phosphate of ammonia and magnesia, is one much more perfect; the whole of the magnesia appears to be precipitated, and as a method, therefore, of determining the quantity of this

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base, it is probably unexceptionable. It does not, however, altogether accord with the object of the present formula. The soda of the phosphate of soda serves to neutralize the muriatic acid of the muriate of magnesia; a quantity of muriate of soda is of course formed, which remains with the muriate of soda of the water, and the amount of which, therefore, it is necessary to determine with accuracy. This may be done from the quantity of phosphate of magnesia obtained giving the equivalent portion of muriate of soda, either by means of the equivalents of the acids, or of the bases. But still this renders the method somewhat complicated; and it may be liable to some error, if any excess of phosphate of soda be added, which, in order to precipitate the magnesia entirely, it may be difficult to avoid; this excess remaining with the muriate of soda, and rendering the estimate of it incorrect. And independent of these circumstances, it would be preferable to give uniformity to the operation, by employing some method by which the product in this, as well as in the previous steps, is removed, at the end of the analysis, leaving only the muriate of soda.

It seemed probable that this might be attained, by employing phosphoric acid with the carbonate of ammonia, to form the triple phosphate of ammonia and magnesia, such an excess of ammonia being used, as should both be sufficient for the constitution of this compound, and for the neutralization of the muriatic acid of the muriate of magnesia; muriate of ammonia would thus be substituted, the same as in the preceding step of precipitating the lime, which at the end would be expelled by heat, leaving muriate of soda alone. I accordingly found, that when this variation of the process was employed, the clear liquor, after the precipitation, was not affected by the addition either of phosphate of soda with ammonia, or of subcarbonate of soda,—a proof that the separation of the magnesia had

had been complete. To establish its accuracy with more certainty, the following experiments were also made.

Twenty grains of muriate of soda (pure rock-salt), which had been exposed to a red heat, and ten grains of crystallised muriate of magnesia, were dissolved in an ounce of water, at the temperature of 100°. The phosphate of soda and carbonate of ammonia were then employed to precipitate the magnesia in the mode proposed by Dr Wollaston, that is, a solution of the ammoniacal carbonate was first added, and afterwards a solution of phosphate of soda, as long as any precipitation was produced, taking care to preserve in the liquor a slight excess of the ammonia. The precipitate being washed and dried, afforded, after exposure to a red heat for an hour, 5.4 grains of phosphate of magnesia, equivalent to 2.15 of magnesia. The clear liquor being evaporated, muriate of soda was obtained, which, after exposure to a red heat, weighed 25.7 grains. Phosphate of magnesia being composed of 39.7 of magnesia, with 60.3 of phosphoric acid, 5.4 grains of it are equivalent to 6.4 grains of muriate of soda, and this deducted from the quantity obtained 25.7, leaves 19.3 as the quantity originally dissolved.

A solution perfectly the same was prepared, and a solution of carbonate of ammonia was added to it as before. A strong solution of phosphoric acid was then dropped in, as long as any precipitation was produced, observing the precaution of having always an excess of ammoniacal carbonate in the liquor. The precipitate being washed and dried, afforded, after exposure to a red heat, 5.5 grains of phosphate of magnesia equivalent to 2.19 of magnesia. The clear liquor being evaporated, and the dry matter being exposed to a heat gradually raised to redness, weighed, when cold, exactly 20 grains.

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In both experiments, the quantity of muriate of soda is accurately obtained, or as nearly so as can be expected. They correspond, too, as nearly as can be looked for, even in a repetition of the same experiment, in the quantity of magnesia which they indicate. To ascertain how far this corresponded with the real quantity, I converted 10 grains of the crystallised muriate of magnesia into sulphate by the addition of sulphuric acid, and exposed it to a low red heat; the product weighed 6.4 grains, equivalent to 2.13 of magnesia. This may be regarded as a perfect coincidence, and as establishing the accuracy of the other results *.

It thus appears, that phosphoric acid with an excess of ammonia may be employed to precipitate magnesia from its saline combinations; and in a process such as the present, it has the advantage, that the muriate of ammonia formed, can be afterwards volatilised by heat, and the quantity of any residual ingredient can of course be easily ascertained. Neutral phosphate of ammonia would also have this advantage; but it does not succeed, phosphate of magnesia not being sufficiently insoluble. On adding a solution of phosphate of ammonia to a solution of sulphate of magnesia, the mixture became turbid in a minute or two, and in a short time a precipitate in crystalline grains formed at the bottom and sides; but it was not considerable, and did not increase. Phosphate of ammonia, however, with an excess of ammonia, or with the previous addition of carbonate of ammonia, may be employed with the same

* According to the result of this last experiment, 100 grains of crystallised muriate of magnesia would give 64 of real sulphate of magnesia, composed of 21.3 of magnesia, and 42.7 of sulphuric acid. This quantity of sulphuric acid is equivalent to 29.4 of muriatic acid. Hence 100 grains of this salt crystallised (in which state its composition, I believe, has not been determined) consist of 21.3 magnesia, 29.4 muriatic acid, and 49.3 of water.

same effect as phosphoric acid. In applying the phosphoric acid to this purpose under any of these forms, it is necessary to be careful that it be entirely free from any impregnation of lime.

There is one other advantage which this method has, that if even a slight excess of phosphoric acid be added, the error it can introduce must be extremely trivial; for the effect of it will be only to decompose a small portion of the original muriate of soda; and as the difference is very inconsiderable in the proportion in which phosphoric and muriatic acids combine with soda, any difference of weight which may arise from this substitution, to any extent to which it can be supposed to happen, may be neglected as of no importance *.

To

* For the sake of comparison, and to ascertain the accuracy of different methods, I submitted a similar solution of muriate of magnesia and muriate of soda to analysis by sub-carbonate of ammonia. To the saline liquor, heated to 100°, a solution prepared by dissolving carbonate of ammonia in water of pure ammonia, was added, until it was in excess. A precipitation rather copious took place; the precipitate being collected on a filtre, the clear liquor was evaporated to dryness, and the saline matter was exposed to heat, while any vapours exhaled. Being redissolved, a small portion remained undissolved, and on again adding sub-carbonate of ammonia to the clear liquor, precipitation took place, rather less abundant than at first. This was repeated for a third, and even for a fourth time, after which the liquor was not rendered turbid. Being evaporated, the muriate of soda obtained, after exposure to a red heat, weighed 20.5 grains. The whole precipitate washed, being heated with sulphuric acid, afforded of dry sulphate of magnesia 4.8 grains, a quantity inferior to that obtained by the other methods, evidently owing to the less perfect action of the ammoniacal carbonate as a precipitant. A similar deficiency in the proportion of magnesia was found in the analysis of sea-water by sub-carbonate of ammonia, as has been already stated; while, on the other hand, in its analysis by phosphate of soda and carbonate of ammonia, a larger quantity of muriate of soda was obtained than by the other methods, probably from the difficulty of avoiding an excess of phosphate of soda in precipitating the magnesia.

To apply this method, then, to the present formula; add to the clear liquor poured off after the precipitation of the oxalate of lime, heated to 100°, and, if necessary, reduced by evaporation, a solution of carbonate of ammonia; and immediately drop in a strong solution of phosphoric acid, or phosphate of ammonia, continuing this addition with fresh portions, if necessary, of carbonate of ammonia, so as to preserve an excess of ammonia in the liquor as long as any precipitation is produced. Let the precipitate be washed; when dried by a heat not exceeding 100°, it is the phosphate of ammonia and magnesia containing .019 of this earth; but it is better for the sake of accuracy, to convert it into phosphate of magnesia by calcination for an hour at a red heat: 100 grains, then, contain 40 of magnesia.

Evaporate the liquor remaining after the preceding operations to dryness, and expose the dry mass to heat as long as any vapours exhale, raising it towards the end to redness. The residual matter is muriate of soda, 100 grains of which are equivalent to 53.3 of soda, and 46.7 of muriatic acid. It is not, however, to be considered necessarily as the quantity of muriate of soda contained in the water; for a portion of soda may have been present above that combined with muriatic acid, united, for example, with portions of sulphuric or carbonic acid; and, from the nature of the analysis, this, in the progress of it, or rather in the first step, that of the removal of these acids by the muriate of barytes, would be combined with muriatic acid. It does not, therefore, give the original quantity of that acid; but it gives the quantity of Soda, since no portion of this base has been abstracted, and none introduced.

The quantity of muriatic acid may have been either greater or less than that in the muriate of soda obtained. If the quantity

tity of soda existing in the water exceeded what the proportion of muriatic acid could neutralise, this excess of soda being combined with sulphuric or carbonic acid, then, in the removal of these acids by muriate of barytes, muriatic acid would be substituted, which would remain in the state of muriate of soda; and if the quantity considered as an original ingredient were estimated from the quantity of this salt obtained, it would be stated too high. Or if, on the other hand, more muriatic acid existed in the water than what the soda present could neutralise, the excess being combined with the other bases, lime or magnesia, then, as in the process by which these earths are precipitated, this portion of the acid would be combined with ammonia, and afterwards dissipated in the state of muriate of ammonia, if the original quantity, were inferred from the weight of the muriate of soda obtained, it would be stated too low.

To find the real quantity, therefore, another step is necessary. The quantities of bases, and of acids procured, (taking the quantity of muriatic acid existing in the muriate of soda obtained), being combined according to the known proportions of their binary combinations, if any portion of muriatic acid has been abstracted, the bases will be in excess, and the quantity of this acid necessary to produce neutralization, will be the quantity lost; or, on the other hand, if any portion of muriatic acid has been introduced, and remains beyond that originally contained in the water, this quantity will be in excess above what is necessary to produce neutralization. The simple rule, therefore, is to combine the elements obtained by the analysis, in binary combinations, according to the known proportions in which they unite; the excess or deficiency of muriatic acid will then appear; and the amount of the excess being subtracted from the quantity of muriatic acid contained

 in

in the muriate of soda obtained, or the amount of the deficit being added to that quantity, the real quantity of MURIATIC ACID will be obtained *.

There is one deficiency, however, in this method. If any error has been introduced in any previous step of the analysis, either in the estimation of the bases or of the acids, this error will be concealed by the kind of compensation that is made for it, by thus adapting the proportion of muriatic acid, to the results such as they are obtained; and at the same time, an incorrect estimate will be made of the quantity of muriatic acid itself. When any error, therefore, can be supposed to exist, or, independent of this, to ensure perfect accuracy, it may be proper to estimate directly the quantity of muriatic acid in a given portion of the water, by abstracting any sulphuric or carbonic acid by nitrate of barytes, and then precipitating the muriatic acid by nitrate of silver or nitrate of lead. The real quantity will thus be determined with perfect precision, and the result will form a check on the other steps of the analysis, as it will lead to the detection of any error in the estimate of the other ingredients; for when the quantity is thus found, the quantities of these must bear that proportion to it which will correspond with the state of neutralization.

Thus, by these methods, the different acids, and the different bases are discovered, and their quantities determined. To complete the analysis, it remains to infer the state of combination in which they exist. It will probably be admitted, that this must be done on a different principle from that on which the composition of mineral waters has hitherto been inferred. The compounds which may be obtained by direct analysis, cannot

* The analysis of sea-water in a preceding paper, will afford an illustration of this (page 237.)

cannot be considered as being necessarily the real ingredients, and to state them as such would often convey a wrong idea of the real composition. There are two views according to which the state of combination in a saline solution may be inferred, and in conformity to which, therefore, the composition of a mineral water may be assigned. It may be supposed, that the acids and bases are in simultaneous combinations. Or if they be in binary combinations, the most probable conclusion with regard to this, as I have already endeavoured to shew, (p. 230.) is, that the combinations are those which form the most soluble compounds, their separation in less soluble compounds, on evaporation, arising from the influence of the force of cohesion. In either of these cases, the propriety of first stating as the results of analysis the quantities of acids and bases obtained, is obvious. On the one supposition, that of their existing in simultaneous combination, it is all that is to be done. On the other supposition, the statement affords the grounds on which the proportions of the binary compounds are inferred. And there can be no impropriety in adding the composition conformable to the products of evaporation. The results of the analysis of a mineral water may always be stated, then, in these three modes: 1*st*, The quantities of the acids and bases: *2dly*, The quantities of the binary compounds, as inferred from the principle, that the most soluble compounds are the ingredients; which will have at the same time the advantage of exhibiting the most active composition which can be assigned, and hence of best accounting for any medicinal powers the water may possess: And, *3dly*, The quantities of the binary compounds, such as they are obtained by evaporation, or any other direct analytic operation. The results will thus be presented under every point of view.

It is obvious that the process I have described, adapted to the most complicated composition which usually occurs, is to be modified according to the ingredients. If no lime, for example, is present, then the oxalate of ammonia is not employed; and in like manner with regard to the others. I have also supposed the usual and obvious precautions to be observed, such as not adding an excess of any of the precipitants, bringing the products to a uniform state of dryness, &c. having mentioned only any source of error less obvious, or peculiar to the process itself.

With regard to other ingredients, either not saline, or more rarely present, it will in general be preferable, when their presence has been indicated by the employment of tests, or by results occurring in the analysis itself, not to combine the investigation to discover them with the general process above described, but to operate on separate portions of the water, and to make the necessary allowance for their quantities in estimating the other ingredients. The quantity of iron, for example, in a given portion of the water, may be found by the most appropriate method. Silica will be discovered by the gelatinous consistence it gives on evaporation, and forming a residue insoluble in acids, but dissolved by a solution of potash. Alumina may be discovered in the preliminary application of tests, by the water giving a precipitate with carbonate of ammonia, which is not soluble, or is only partially soluble in weak distilled vinegar, but is dissolved by boiling in a solution of potash, or by its precipitation from the water sufficiently evaporated by succinate of soda; or in conducting the process itself, it will remain in solution after the precipitation of the lime by the oxalic acid, and be detected by the turbid appearance produced on the addition of the carbonate of ammonia previous to the addition of the phosphoric acid to discover the

magnesia.

magnesia. Its quantity may then be estimated from its precipitation by carbonate of ammonia, or by other methods usually employed. Silica will also be precipitated in the same stage of the process; its separation from the alumina may be effected by submitting the precipitates, thoroughly dried, to the action of diluted sulphuric acid. Potash when present, which is very seldom to be looked for, will remain at the end, in the state of muriate of potash. Muriate of platina will detect its presence, and the muriate of potash may be separated by crystallization from the muriate of soda.

There is another mode in which part of the analysis may be conducted, which, although perhaps a little less accurate than that which forms the preceding formula, is simple and easy of execution, and which may hence occasionally be admitted as a variation of the process; the outline of which, therefore, I may briefly state.

The water being partially evaporated, and the sulphuric and carbonic acids, if they are present, being removed by the addition of muriate of barytes, and the conversion of the whole salts into muriates effected in the manner already described; the liquor may be evaporated to dryness, avoiding an excess of heat, by which the muriate of magnesia, if present, might be decomposed; then add to the dry mass six times its weight of rectified alcohol (of the specific gravity at least of .835), and agitate them occasionally during twenty-four hours, without applying heat. The muriates of lime and magnesia will thus be dissolved, while any muriate of soda will remain undissolved.

ved. To remove the former more completely, when the solution is poured off, add to the residue about twice its weight of the same alcohol, and allow them to stand for some hours, agitating frequently. And when this liquor is poured off, wash the undissolved matter with a small portion of alcohol, which add to the former liquors.

Although muriate of soda by itself is insoluble, or nearly so, in alcohol of this strength, yet when submitted to its action along with muriate of lime or of magnesia, a little of it is dissolved. To guard against error from this, therefore, evaporate or distil the alcoholic solution to dryness, and submit the dry mass, again, to the action of alcohol in smaller quantity than before; any muriate of soda which had been dissolved will now remain undissolved, and may be added to the other portion; or at least any quantity of it dissolved must be extremely minute. A slight trace of muriate of lime or of magnesia may adhere to the muriate of soda, but when a sufficient quantity of alcohol has been employed, the quantity is scarcely appreciable; and the trivial errors from these two circumstances counteract each other, and so far serve to give the result more nearly accurate.

Evaporate the alcohol of the solution, or draw it off by distillation. To the solid matter add sulphuric acid, so as to expel the whole muriatic acid; and expose the residue to a heat approaching to redness, to remove any excess of sulphuric acid. By lixiviation with a small portion of water, the sulphate of magnesia will be dissolved, the sulphate of lime remaining undissolved, and the quantities of each, after exposure to a low red heat, will give the proportions of lime and magnesia. The quantity of soda will be found from the weight of the muriate of soda heated to redness; and the quantities of the acids will be determined in the same manner as in the general formula.

This

This method is equally proper to discover other ingredients which are more rarely present in mineral waters. Thus, alumina will remain in the state of sulphate of alumina along with the sulphate of magnesia, and may be detected by precipitation by bi-carbonate of ammonia. Silica will remain with the muriate of soda after the action of the alcohol, and will be obtained on dissolving that salt in water. And iron will be discovered by the colour it will give to the concentrated liquors, or the dry residues, in one or other of the steps of the operation.

The general process I have described may be applied to the analysis of earthy minerals. When they are of such a composition as to be dissolved entirely, or nearly so, by an acid, that is, where they consist chiefly of lime, magnesia, and alumina, its direct application is sufficiently obvious; where they require the previous action of an alkali from the predominance of siliceous earth, on this being separated, the excess of alkali may be neutralised by muriatic acid; and the remaining steps of the analysis may be prosecuted, with any modification which the peculiar composition will require. As the quantities of the ingredients are capable of being estimated with so much precision, it may be employed with more peculiar advantage where a small quantity only of the mineral can be submitted to analysis; and when it is employed, such a quantity only, ten grains, for example, ought to be made the subject of experiment.

END OF PART FIRST.

XV. *On the Effects of Compression and Dilatation in altering the Polarising Structure of Doubly Refracting Crystals.* *By* DAVID BREWSTER, LL. D. F. R. S. LOND. & EDIN.

(*Read November* 17. 1816.)

IN a paper which I had the honour of submitting to the Society, at the end of last session, I gave a detailed account of the effects of mechanical compression and dilatation, in communicating to glass and other uncrystallised bodies, all the properties of doubly refracting crystals. I had at that time attempted, without success, to alter the polarising structure of doubly refracting crystals, although I applied the force of powerful screws to Topaz, Rock-crystal, and Calcareous spar. All the specimens which I employed were crushed to pieces by the pressure, but exhibited no traces of the coloured rings when exposed to polarised light.

The cause of the failure of these experiments did not occur to me, till I was engaged in examining the phenomena produced in the direction of the resultant axes of regular crystals. I then saw, that the pressure formerly applied, had actually developed a new polarising force, but that it had been

 applied

applied at such an angle with the axis, that the compound force arising from the combination of the new force with the ordinary polarising force of the crystal, produced tints far beyond the limits of NEWTON's scale. In order, therefore, to observe the influence of the polarising force generated by pressure, it became necessary either to use a crystallised plate which was so thin as to exhibit tints within the limits of NEWTON's scale, or to apply the force at right angles to the axis, and to transmit the polarised light, either along the axis, or at such an angle with it as corresponded to a tint below the fifth or sixth order.

I therefore took a plate of *Calcareous spar*, (one of the *negative* class of crystals), bounded by planes perpendicular to the axis of double refraction, or to the short diagonal of the primitive rhomb; and having exposed it to polarised light, I observed the beautiful system of circular and highly coloured rings which it produced. The force of a screw was now applied to the sides of the plate, and the rings instantly began to lose their circular shape, to swell and contract in different places, and to bend into curves of contrary flexure at the points of pressure. By continuing the pressure, the plate was broken to pieces.

Instead of grinding down the opposite obtuse solid angles of the rhomboid of calcareous spar, I took a complete rhomboid, and cemented upon two of its parallel surfaces a prism of flint-glass, whose refracting angle was 45°. When a polarised ray was incident almost vertically upon one of the faces of the prism, so as to be refracted parallel to the diagonal of the rhomb, the system of coloured rings was distinctly seen. I now pressed together, by means of screws, the other four parallel surfaces of the rhomboid, and observed in a very satisfactory manner the change of form induced upon the circular system of rings.

Similar

Similar experiments were made with *Quartz*, a crystal of the *positive* class, and with various other crystals, both with one and two axes of double refraction; and in every case the tints were either raised or depressed in the scale of colours. The same effects were obtained at various angles with the axis, either by reducing the thickness of the plates, or by bringing the tints within the limits of NEWTON'S scale, by the opposite action of plates of sulphate of lime.

We may therefore consider it as an established fact, that the phenomena produced by the polarising forces of all crystals, whether they have one or more axes, and whether their action is positive or negative, are very considerably affected by subjecting the crystals to compressing or dilating forces.

The effect which we have now described may arise from two causes, either from an actual modification of the original polarising force of the crystal, or from the developement of a new force, which merely combines its effects with those of the original force. The first of these cases is exemplified, when we subject to pressure a plate of glass along which heat is in the act of being transmitted. The pressure which is thus applied, alters the state of aggregation into which the glass is thrown by the passing heat, and produces a real modification of its former polarising force. When, on the other hand, we combine a plate of sulphate of lime with a plate of calcareous spar, the resulting tint arises merely from a combination of the tints which these crystals produce separately; the polarising force of the calcareous spar remaining the same as before.

In order to determine whether pressure modifies the original force, or creates a new one in doubly-refracting crystals, I cut the crystals into different shapes, and found that the effect produced by pressure varied with the external shape of

the specimen, in the same manner as in plates of glass. Hence it follows, that since the polarising force of crystals is in no respect influenced by their external shape, a new and moveable polarising force is generated by pressure, which increases or diminishes the effect of the permanent force, according to the direction in which it is applied. The effect, therefore, of a crystallised plate subjected to pressure, is the same as if we combined it, when free from pressure, with a similar plate of the same substance, destitute of any polarising force, and pressed in a similar manner. The force residing in the ultimate particles of the crystal is unchangeable, and the pressure developes the new force, by merely altering their state of aggregation.

With the aid of these views, we may now predict all the changes which can be produced upon positive and negative crystals, by mechanical compression and dilatation.

When the two parallel surfaces of a transparent solid are brought nearer each other by pressure, the tint, in the direction of a line perpendicular to these surfaces, which may be called the *axis of compression*, is negative, and therefore the polarising force produced by compression is *negative*, like that of *calcareous spar*, &c. When the two surfaces are, on the other hand, separated from each other by dilatation, the tint in the direction of the perpendicular, which may be called the *axis of dilatation*, is negative, and consequently the polarising force produced by dilatation is positive, like that of *zircon*, &c.

Hence, if we take plates of crystals, and apply the forces to parallel surfaces, we shall obtain the results contained in the following Table:

TABLE

TABLE, *shewing the Effect of Compression and Dilatation upon Positive and Negative Crystals.*

	Axis of Compression or Dilatation parallel to the Axis of the Crystal.	Axis of Compression or Dilatation perpendicular to the Axis of the Crystal.
Positive crystals, compressed,	Tints rise,	Tints descend.
——— ——— dilated,	Tints descend,	Tints rise.
Negative crystals, compressed,	Tints descend,	Tints rise.
——— ——— dilated,	Tints rise,	Tints descend.

Now, since every compression is accompanied with a dilatation in a direction perpendicular to the axis of compression, and *vice versa*, it is obvious from the Table, that these simultaneous changes in the state of aggregation of the particles, will act in combination, that is, they will conspire in producing either a positive or a negative polarisation.

The preceding results, which are deducible, *a priori*, from the principles already established, I have confirmed by direct experiments upon calcareous spar, quartz, and various other doubly-refracting crystals, cut into different shapes ; and I have obtained analogous results, by the application of pressure to crystals with two axes of extraordinary refraction.

If the axis of compression is perpendicular to the axis of double refraction, the crystal is converted into a crystal with two axes, the poles of the two resultant axes or diameters of no polarisation being distinctly visible.

The

The tints discovered by M. Biot along the axis of Rock Crystal, and supposed to arise from a rotatory motion of the particles of light, suffer no other change from pressure, than that which they experience by being combined with the tints produced by any other crystal.

When the state of aggregation of the particles of doubly-refracting crystals, is altered by the agency of heat, their polarising forces suffer analogous changes; but these changes are less perfectly developed than in glass, owing to the great conducting power of regularly crystallised bodies.

XVI.

XVI. *Experiments on Muriatic Acid Gas, with Observations on its Chemical Constitution, and on some other Subjects of Chemical Theory.* *By* JOHN MURRAY, M. D. F. R. S. E.

(Read 15th Dec. 1817, *and* 12*th Jan.* 1818.*)*

PART I.

SOME years ago I proposed, as decisive of the question which has been the subject of controversy on the nature of Oxymuriatic and Muriatic Acids, the experiment of procuring water from muriate of ammonia, formed by the combination of dry ammoniacal and muriatic acid gases. Muriatic acid gas being the sole product of the mutual action of oxymuriatic gas and hydrogen, it follows, that if oxymuriatic gas contain oxygen, muriatic acid gas must contain combined water; while, if the former be a simple body, the latter must be the real acid, free from water. When muriatic acid gas is submitted to the action of substances which combine with acids, water is obtained; but though the most simple and direct conclusion from this is, that the water is deposited from the muriatic acid gas, the result may be accounted for on the opposite doctrine, by the

the supposition, that it is water formed by the combination of the hydrogen of the acid with the oxygen of the base. Ammonia, however, containing no oxygen, if water is obtained from its combination with muriatic acid gas, we obtain a result which cannot be accounted for on this hypothesis, but must be regarded as a proof of the presence of water in the acid gas. And this, again, affords a proof equally conclusive of the existence of oxygen in oxymuriatic gas.

The results of the experiment which I had brought forward, were involved in much controversial discussion: And a brief recapitulation of the objections that were urged to it, is necessary, as an introduction to the experiments I have now to submit; and to the consideration of the present state of the question.

The original experiment was performed by combining thirty cubic inches of muriatic acid gas, with the same volume of ammoniacal gas carefully dried. The salt formed was exposed in a small retort with a receiver adapted to it, to a moderate heat gradually raised. Moisture speedily condensed in the neck of the retort, which increased and collected into small globules *.

This result was admitted by those who defended the new doctrine, when the experiment was performed in the manner I have described,—water being obtained, it was allowed "in no inconsiderable quantity." But, to obviate the conclusion, it was asserted, that this is water which has been absorbed by the salt from the atmosphere. This was affirmed by Sir Humphry Davy, who stated that the salt absorbs water in this manner to a very considerable extent; that it is only from the salt in this state that water can be procured, and that when it is formed from the

* Nicholson's Journal, vol. xxxi. p. 126.

the combination of the gases in a close vessel, and heated without exposure to the air, not the slightest trace of water appears, even when the experiment is performed on a large scale.

The reverse of this I was able to demonstrate by farther experimental investigations. It was shewn, that the salt absorbs no moisture from the air in the common state of dryness and temperature in which the experiment is performed: when weighed immediately on its formation, in an exhausted vessel, it gains no weight from exposure, but remains the same after a number of hours; and when exposed to the air in the freest manner, it remains, after many days, perfectly dry. It was farther shewn, that when the other circumstances of the experiment are the same, it yields no larger portion of water when it has been exposed to the air, than it does without this previous exposure. And, lastly, it was proved, that when the salt has been formed, and is heated without the air having been admitted, water is obtained from it. This last result was even at length admitted by those who had advanced the opposite assertion, in an experiment performed with a view to determine the fact. The quantity of water was indeed less than what is procured in the other mode; but this was obviously owing to the circumstances of the experiment being unfavourable to its expulsion,—more particularly to the difficulty of applying a regulated temperature to a thin crust of salt, so as to separate the water without volatilising the salt itself,—and to the effect arising from the whole internal surface of a large vessel being encrusted with the salt, so that if the heat is locally applied, the aqueous vapour expelled from one part is in a great measure condensed and absorbed at another, or if the heat is applied equally, is retained in the elastic form, and, as it is cooled, is equally condensed. Accordingly, when the experiment

was repeated, obviating these sources of error as far as possible, the water obtained was in larger quantity. And as no fallacy belongs to the conducting the experiment in the more favourable mode in which it was first performed, (the assertion of the absorption of water from the air being altogether unfounded), the quantity procured in that mode is to be regarded as the real result *.

The argument was maintained, that the water might be derived from hygrometric vapour in the gases submitted to experiment. This it was easy to refute. Dr HENRY had shewn, that ammonia after exposure to potash, and muriatic acid after exposure to muriate of lime, retain no trace of vapour whatever. And these precautions had been very carefully observed. The assertion was brought forward, too, only to account for the minute quantity of water obtained in that mode of conducting the experiment which affords the least favourable result, and were it even admitted to all the extent to which it can be supposed to exist, is inadequate to account for the larger quantity obtained in the other.

That the entire quantity of water contained in the muriatic acid gas, is not to be looked for, is evident from the nature of the ammoniacal salt, particularly its volatility, whence the due degree of heat to effect the separation of the water cannot be applied. If the other muriates yield the greater part of their water, only when raised nearly to a red heat, (which is the case), it is not to be supposed that muriate of ammonia shall do so at a temperature so much lower, as that which it can sustain without volatilization. What is to be expected, is a certain portion of water, greater as the arrangements employed are better adapted to obviate the peculiar difficulty attending the

* NICHOLSON's Journal, vol. xxxii. p. 186, &c.; vol. xxxiv. p. 271.

the experiment. There *is* a production of water in every form of it; and there exists no just argument whence it can be inferred, that the quantity is less than what ought to be obtained. On the opposite doctrine, none whatever should appear.

To effect the more perfect separation of the water from the muriate of ammonia, I had performed the additional experiment of passing the salt formed from the combination of the two gases, in vapour through ignited charcoal, on the principle that by the interposition of the charcoal, the transmission of the vapour would be impeded, and it would be exposed to a more extensive surface, at which a high temperature would operate, while some effect might also be obtained from the affinities exerted by the carbonaceous matter. To remove any ambiguity from the effect of the charcoal, it was previously exposed in an iron tube to a very intense heat, until all production of elastic fluid had ceased; and removed, while still warm, into a tube of Wedgwood's porcelain, containing the muriate of ammonia, which was then placed across a furnace, so as to be raised to a red heat. As soon as the vapour of the salt passed through the ignited charcoal, gas was disengaged, which was conveyed by a curved glass tube adapted to the porcelain one, and received in a jar over quicksilver. Moisture was at the same time pretty copiously deposited, condensing both in the glass-tube in globules, and being brought in vapour with the gas which it rendered opaque, and condensing on the surface of the quicksilver within the jars. The elastic fluid consisted of carburetted hydrogen, and carbonic acid, products evidently of the decomposition by the ignited charcoal of a portion of the liberated water. In this experiment, then, the result was still more satisfactory than in the other. That no ambiguity arose from any effect of the charcoal in affording

 water,

water, is evident from this, that the water appeared at the moment the salt began to pass in vapour, and at a temperature far below that at which the charcoal had ceased to afford any gas. In another variation of the experiment, muriate of ammonia was passed in vapour, through an ignited porcelain-tube alone. Water was obtained in larger quantity than when the salt had been exposed to a heat short of its volatilization; and even the salt which had yielded water by that operation, afforded an additional quantity in this mode,—a proof of the more perfect separation of the water by the effect of a higher temperature *.

By all these results, then, I consider the existence of water in muriate of ammonia, and of course in muriatic acid gas, as demonstrated.

Dr Ure has lately laid before the Society the result of another mode of conducting the experiment,—that of subliming the muriate of ammonia over some of the metals, at the temperature of ignition. Water is thus stated to be obtained in considerable quantity, with a production of hydrogen gas.

No objection appeared to Dr Ure's experiment, except, perhaps, that the salt operated on, was not that formed by the direct combination of its constituent gases, but the common sal ammoniac, in which water might be supposed to exist, either as an essential, or an adventitious ingredient, as it is abundantly supplied to it in the processes by which it is formed. I had found, indeed, in some of my former experiments †, that sal ammoniac yields no water when exposed to a heat sufficient to sublime it, but affords it only when exposed to a red heat by transmission of its vapour through an ignited tube,—that, therefore, (owing no doubt to its previous sublimation,) it contains

* Nicholson's Journal, vol. xxxi. p. 129.

† *Id.* vol. xxxiv. p. 274.

tains apparently even less water than the salt formed by the combination of the two gases. Still, objections entitled to less consideration than this one, had been maintained in the course of this controversy. I therefore thought it right to repeat the experiment, with the necessary precaution to obviate it, and to observe the actual result.

Thirty grains of muriate of ammonia, formed from the combination of muriatic acid and ammoniacal gases, were put into a glass tube with a slight curvature. Two hundred grains of clean and dry iron filings were placed over it. The tube was put in a case of iron with sand, and placed across a small furnace, so that the middle part, where the iron filings were, was at a red heat, the extremity terminating in the mercurial trough. The salt, from the heat reaching the closed extremity of the tube, soon passed in vapour through the ignited iron. Gas issued from the extremity, and moisture appeared in the cold part of the tube. A large quantity of gas was collected, which had the odour quite strong of muriatic acid, and was in part condensed by water; the residue burned with the flame of hydrogen. The tube, for several inches, was studded with globules of water, and was bedimmed with vapour farther. I did not prosecute the experiment, so as to ascertain the weight of water produced, as I had other experiments in view, which I conceived might afford more conclusive results. But it proves the point it was designed to establish, that water is obtained from the salt formed by the combination of the gases, as well as from the common sal ammoniac.

My attention having been thus recalled to the subject, I have again executed the experiment in its original and simplest form,—that of obtaining water from the salt by heat alone; and to this I was led more particularly, as it had occurred to me, that a more perfect abstraction of its water might be effected,

fected, by conducting the experiment in an apparatus somewhat on the principle of the instrument invented by Dr WOLLASTON, which he named the Cryophorus. In a retort of the capacity of seven cubic inches, fitted with a stop-cock, and exhausted, sixty cubic inches of ammoniacal gas were combined with the requisite quantity of muriatic acid gas, each previously carefully dried,—the former by exposure to potash, the latter by exposure to muriate of lime. The stop-cock was then detached from the retort; the excess of ammoniacal gas was removed by a caoutchouc bottle, and replaced by atmospheric air; the salt was pushed down from the neck; and it was connected with another similar retort, the joining of the two being secured by cement. This last retort was also fitted with a stop-cock adapted to a tubulature at its curvature, and heat being applied to it, a little of the included air was allowed to escape. It was then placed in a mixture of muriate of lime and ice, while the other, containing the muriate of ammonia, was placed in warm oil. The heat of this was raised to 420° of Fahrenheit: moisture condensed at the upper part of the neck, when the heat had been raised to 220°, and continued for some time to increase. It then diminished, from the continued application of the heat, carrying it forward into the cold retort, and at the end of the experiment a considerable part of the body of this was encrusted with a thin film of ice. This result, therefore, coincides entirely with what had been before obtained *.

Another

* A foreign chemist, who has continued to support the old doctrine of the nature of muriatic acid, has observed, (*Annals of Philosophy*, vol. viii. p. 204.) that the water of the muriatic acid gas cannot be supposed to be obtained by the combination of the acid with ammonia, for no neutral ammoniacal salt, he adds, can be obtained free from water, and the water of the acid gas becomes the water

Another form of experiment occurred to me still more direct and simple, that of transmitting muriatic acid in its gaseous form over ignited metals. If water be obtained in this experiment, it is a result which would prove subversive of the new doctrine; for muriatic acid gas is held to be the real acid, free from water, and the only change which can happen, is that [illegible] of

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ter essential to the salt. I did not think it necessary to make any reply to this observation, founded entirely, as it appeared to me, on a mistaken assumption. But I may take this opportunity of remarking, that there is no necessary truth in the supposition that the ammoniacal salts must contain water which they cannot yield. When acids combine with bases, the water of the acid does not necessarily remain in the compound. On the contrary, it is capable of being driven off from the greater number of them, by an elevated temperature; and there is no principle on which it can be inferred, that ammonia should in this respect be different from other bases. That it is incapable, as the same chemist remarks, (*Annals*, vol. vii. p. 434.) of combining with a dry acid, so as to form a neutral compound, is of no weight; for the same thing is true of other bases, which yet, when combined with such an acid by the aid of water, allow this water to escape from the combination. He himself observes, that well-burnt lime, free from water, does not absorb dry carbonic acid gas, but absorbs it rapidly if aqueous vapour be admitted, though water is not retained in the composition of carbonate of lime. And I have found, that dry magnesia does not absorb muriatic acid gas, though with the aid of water it forms a combination from which the water can be expelled by heat. That ammoniacal salts exist without water, is evident from the combination of carbonic acid gas and ammoniacal gas, being effected with the greatest facility; and the circumstance that this compound is not neutral, is one not depending on the peculiarity of the ammonia, and its not containing water, like other bases, but on that of the carbonic acid, which, with all the alkalis, even where water is present, has a tendency to form compounds with excess of base. The reason why the ammoniacal salts do not yield the combined water of their acids so completely as that of other salts, is, that from their volatility, or their susceptibility of decomposition, they do not bear that degree of heat which is necessary to produce it. I cannot, therefore, but consider the observation alluded to, as one altogether unfounded, and which ought not on mere speculation to have been brought forward against a positive result.

of the metal decomposing the acid, attracting its chlorine and liberating its hydrogen. And the experiment is farther free from the only resource which remained to the advocates of that doctrine, in the case of water being obtained from muriate of ammonia, that it might be derived from the decomposition of the elements of ammonia, regarding it as an alkali containing oxygen. If water were really obtained from the combination of muriatic acid and ammoniacal gases, it would rather indicate, it was said, the decomposition of nitrogen than the existence of water as a constituent of muriatic acid. No weight, I believe, is due to such an assumption, but if any importance were attached to it, it is precluded if water is obtained from the action of metals on muriatic acid gas.

I have executed the experiment in several forms; and in all with a more or less satisfactory result.

One hundred grains of iron filings, clean and dry, were strewed for a length of five or six inches, in a glass-tube which was placed in an iron case, across a small furnace, so as to admit of being raised to a red heat. This tube, of about two feet in length, was connected with a wide tube eight inches long, containing dry and warm muriate of lime; and this was farther connected, at its other extremity, with a retort affording muriatic acid gas, from a mixture of super-sulphate of potash and muriate of soda. The open extremity of the long tube, dipped by a slight curvature in quicksilver. On the iron being raised to ignition, and the transmission of the acid gas being conducted slowly, elastic fluid escaped from the extremity of the tube, which was found to be hydrogen, and though no trace of moisture appeared in the anterior part of the tube, it immediately condensed in that part which was cold, beyond the iron filings. This accumulated in globules, and at length run

run into a small portion in the bottom; the sides were bedewed for a length of six inches, and a thin film of moisture appeared beyond, nearly its whole length.

By the muriatic acid gas being extricated in the preceding experiment from nearly dry materials, and by its previous transmission over an extensive surface of loose muriate of lime, it was inferred, that it would be free from hygrometric vapour; and that it held no moisture, was apparent from no trace of it appearing in the anterior portion of the tube. To obviate, however, entirely, any supposed fallacy from this source, the experiment was performed in the following manner. One hundred grains of clean and perfectly dry iron filings were put into a long glass tube, which was placed, as before, across a small furnace. Muriatic acid gas had been kept in contact with dry muriate of lime for three days, in a jar with a stopcock adapted to it. This was connected, by a short tube with a caoutchouc collar, with the tube containing the iron filings; and a little of the muriatic acid gas being passed through the tube to expel the air, the temperature was raised to ignition. The slow transmission of the gas was continued by the pressure of the mercury in the quicksilver trough, and fresh quantities, which had been equally with the other exposed to muriate of lime, were added, as was necessary. Water almost immediately appeared in the tube beyond the iron filings, it collected in spherules, and continued to accumulate as the gas continued to be transmitted for a length of about seven inches. A portion of the gas which escaped from the extremity, was clouded, and deposited a film of moisture on the sides of the jar in which it was received over quicksilver. The quantity of gas transmitted amounted to about thirty-five cubic inches.

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There are some difficulties in conducting the experiment in the manner now described, from the consolidation of the metallic matter, and the volatilization of the product. It was also of some importance to vary the experiment. I therefore performed it in another mode. Metals scarcely act on muriatic acid gas, at natural temperatures, but from such a degree of heat as could be applied by a small lamp, both iron and zinc were acted on; the gas suffered diminution of volume, hydrogen was formed, and a sensible production of moisture took place. The simplest mode of exhibiting this, is to introduce iron or zinc filings, previously dry, and warm, into a retort fitted with a stop-cock; exhausting it; then admitting dry muriatic acid gas; and applying heat, by a small lamp, to the filings in the under part of the body of the retort. Moisture soon appears at its curvature in small globules, and increases on successive applications of the heat with the admission of the requisite quantities of gas.

To conduct the experiment, however, on a larger scale, I employed a different apparatus. A tubulated retort, of the capacity of twenty-five cubic inches, was connected with a jar, containing muriatic acid gas in contact with muriate of lime, on the shelf of the mercurial trough, by a tube bent twice at right angles, and fitted by its shorter leg with a collar of caoutchouc to a stop-cock at the top of the jar, its longer leg passing into the tubulature of the retort, so as to terminate within an inch of its bottom, and the joinings being rendered air-tight. The retort is so placed, that heat can be applied by a lamp to the bottom, and its neck dips, by a short curved tube, under a jar filled with quicksilver, which, by the reverted position of the retort, may be placed beside the other, on the shelf of the trough. At the commencement of the experiment, the metallic filings, previously dry and warm, having been put into the retort, the atmospheric air is expelled by a moderate heat, and

and small portions of the muriatic acid gas are admitted, until the retort is filled with the pure gas. The stop-cock is then closed, and heat is applied by a lamp to the bottom of the retort, under a considerable pressure of mercury; any small portion of gas, expelled at the extremity, being received in the small jar. The heat can thus be successively cautiously applied, and this, as the experiment proceeds, to a greater extent, in consequence of the diminution of volume that takes place. Fresh quantities of muriatic acid gas are admitted from time to time from the jar, and the stop-cock being closed when the heat is applied, the hydrogen gas produced is expelled, with any muriatic acid gas not acted on.

In the principal experiment I employed, zinc filings were used in preference to iron, from the consideration, that muriate of zinc is less volatile than muriate of iron, and therefore would admit of a higher heat being applied to expel any water. One hundred grains of clean and dry zinc filings were introduced, while warm, into the retort; the air was expelled, and muriatic acid gas was admitted from the jar. On applying heat to the zinc, the retort, which was before perfectly dry, was bedimmed with moisture at its curvature, and small spherules collected at the top of the neck. These increased in size, and extended farther as the experiment advanced. After a certain time, part of this disappeared in the interval of cooling, being absorbed by the deliquescent product; but when the heat was again applied, it was renewed, and this in increased quantity, until at length, at the end of four days, during which heat had been frequently applied, the whole tube of the retort, seven inches in length, was studded with small globules of fluid. When the heat had been raised high, a beautiful arborescent crystallization appeared in a thin film on the body of the retort, but no part of this reached the neck. The retort was now detached; the gas it contained was withdrawn by a caout-

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chouc bottle; a small receiver was adapted; and a slight heat having been applied, to expel a little of the air, the joining was made close by cement. The receiver was surrounded with a freezing mixture, and heat was applied by a choffer to the retort, as far as could be done, without raising dense vapours. Globules of liquid, perfectly limpid, collected pretty copiously towards the middle and lower part of the neck, and the receiver, on being removed from the freezing mixture, was covered internally with a film of moisture. The globules in the neck of the retort were absorbed by a slip of bibulous paper, and the quantity was found to amount to 1.2 gr. The receiver being dried carefully, and weighed, lost by the dissipation of the moisture within, 0.4 grain. Distilled water, in which the bibulous paper was immersed, was quite acid; it gave no sensible turbidness on the addition of ammonia, or of carbonate of soda, and held dissolved, therefore, merely pure muriatic acid. The mass in the retort was of a grey colour, with metallic lustre, in loosely aggregated laminæ, somewhat flexible. It weighed 114.8 grains. Adding to this increase of weight, which the zinc had gained, the weight of the water and the hydrogen gas expelled, it gives a consumption of muriatic acid gas of about 16.8 grains, equivalent to about 43 cubic inches. Supposing the weight of water to be doubled, or nearly so, by saturation with muriatic acid, this gives the product of water in the experiment, as equal to nearly one grain; or about one-fifth of the whole quantity of combined water, which muriatic acid gas is calculated to contain *.

In

* The action of the metals on the muriatic acid gas, taking place in the above experiments at a heat comparatively moderate, it occurred to me, that they might exert a similar action with no higher heat on the acid, in muriate of ammonia,

In all the preceding experiments, water has been procured from muriatic acid gas. It is obvious, that such a result cannot be accounted for on the hypothesis, that it is the real acid free from water, a compound merely of chlorine and hydrogen. On the opposite doctrine, as muriatic acid in its gaseous form is held to contain water, it may be supposed to afford a portion of it.

It may be maintained, however, in this, as it was in the experiment of obtaining water from the muriate of ammonia by heat, that the water produced is derived from hygrometric vapour in the gas. To obviate this, it is sufficient to recur to the fact established by the experiments of HENRY and GAY LUSSAC, that muriatic acid gas contains no hygrometric vapour; and to the obvious result in the experiment, that no quantity that can be assumed, would be adequate to account for the quantity actually obtained. The circumstances of the experiment,

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monia, and that this might afford an easy mode of exhibiting the results. I accordingly found, that on mixing different metals with sal ammoniac in powder, previously exposed to a subliming heat, and exposing the mixture to heat by a lamp, so regulated as to be short of volatilization, the salt was decomposed, ammoniacal gas was expelled, and moisture condensed in the neck of the retort; covering a space of several inches with small globules, and at length running down. The metals I employed were iron, zinc, tin, and lead; 100, 150, or 200 grains of each metal, dry and warm, being mixed with 100 grains of the salt, likewise newly heated. To obviate any fallacy from common sal ammoniac being employed, I repeated the experiment with the salt formed from the combination of its two constituent gases, and obtained the same result. But although this affords an easy mode of exhibiting the production of water, it is not favourable to obtaining a perfect result, the heated ammoniacal gas carrying off a considerable portion of the water deposited; and accordingly, the quantity, instead of increasing as the experiment proceeds, at length diminishes, and the ammoniacal gas deposites a portion of water in passing through mercury, or in being conveyed through a cold tube.

ment, too, are such as to preclude any such supposition; and this more peculiarly so, than in the experiment of obtaining water from the muriate of ammonia by heat; for in the present case, the acid gas is alone employed, while in the other there is an additional equal volume of ammoniacal gas, which may be supposed to afford a double quantity of hygrometric vapour. In the latter, both the gases are condensed into a solid product, and any hygrometric vapour may be supposed to be liberated; but in the present experiment, there remains the hydrogen gas, capable of containing hygrometric vapour, while the muriatic acid gas contains none; and the quantity of it thus transmitted over the humid surface, and expelled from the apparatus, must have carried off more vapour than the other, introduced at a lower temperature, could have conveyed. These circumstances, independent of the quantity of water deposited, precluded the supposition of any deposition from the condensation of hygrometric vapour. And there is no other external source whence it can be derived. In this respect nothing can be more satisfactory than the experiment with the zinc in the apparatus described. The muriatic acid gas rises from dry mercury in contact with muriate of lime, passes through a narrow bent tube, thirty inches in length, without exhibiting the slightest film of moisture, is received into the retort perfectly dry; and when the action of the metal on it is excited by heat, humidity immediately becomes apparent in the curvature of the retort, and this even while the gas is warm, and of course capable of containing more water dissolved, than it could do in its former state; and the quantity increases as the experiment proceeds. No arrangement can be supposed better adapted to prove, that any deposition of water must be by separation from its existence in the gas in a combined state.

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But though I consider this conclusion as established, there is a considerable difficulty attending the theory of the experiment. The result of water being obtained is actually different from what is to be looked for, on the doctrine of muriatic acid gas containing combined water; and even when the fact is established, the theory of it is not easily assigned. On that doctrine, it must be held that in the action of metals on muriatic acid gas, the metal attracts oxygen from the water, the corresponding hydrogen is evolved, and the oxide formed combines with the real acid. No water, therefore, ought to be deposited, for none is abstracted from the acid, but what is spent in the oxidation of the metal. This will be apparent, by attending to the proportions in a single example, from the scale of chemical equivalents: 100 grains of iron combine with 29 of oxygen, and in this state of oxidation unite with 99 of real muriatic acid. This quantity of acid exists in 131.8 of muriatic acid gas, combined with 32.8 of water; and this portion of water contains 29 of oxygen with 3.8 of hydrogen. There is present, therefore, exactly the quantity of oxygen which the metal requires to combine with the acid; and no water remains above this. Or it may be illustrated under another point of view. Muriatic acid gas is composed of oxymuriatic gas and hydrogen. A metal acting on it must attract the oxymuriatic acid,—that is, the muriatic acid and oxygen, and liberate the hydrogen. No water, therefore, ought to appear, more, on this theory, than on the other; but the real products in both must be a dry muriate, or chloride, and hydrogen gas. In the action of ignited metals on muriate of ammonia, it is equally evident, on the same principle, that no water ought to be obtained. How, then, is the production of water to be accounted for?

Though the water obtained in these experiments cannot be derived from hygrometric vapour in the gas, there is another view

view under which it may be regarded as present, as an adventitious ingredient. The acid having a strong attraction to water, may be supposed, in the processes in which it is usually prepared, to retain a portion not strictly essential to its constitution as muriatic acid gas, but still chemically combined,—that is, combined with it with such an attraction as to be liberated only when it passes into other combinations, and it may be this portion which is obtained in the action of metals on the gas; the other portion, that essential to the acid, being sufficient to produce the requisite oxidation of the metal.

The question with regard to the existence of water in this state, GAY LUSSAC and THENARD have already determined. From an extensive series of experiments, they found reason to conclude, that muriatic acid gas, in whatever mode it is prepared, is uniformly the same. From the quantity of hydrogen gas which combines with oxymuriatic gas in its formation, it follows, that it contains 0.25 of water, essential to its constitution. But the gas obtained by the usual processes, afforded, they found, exactly 0.25 of water, when transmitted over oxide of lead, or combined with oxide of silver; and the same compounds are formed, as by the action of oxymuriatic acid on silver and lead in their metallic state. They prepared muriatic acid gas, by heating fused muriate of silver with charcoal moderately calcined. It contained just the same quantity of water as muriatic acid obtained from humid materials, as it afforded the same quantity of hydrogen from the action of potassium. And instead of being capable of receiving the smallest additional portion of water, a single drop of water being introduced into three quarts of it, did not disappear, nor even diminish, but, on the contrary, increased in volume *. These facts

* Recherches Physico-chimiques, t. ii. p. 133.

facts establish the conclusion, that muriatic acid gas can receive no additional portion of water, but that which is essential to it, and hence preclude the solution of the difficulty under consideration by the opposite assumption. And it is to be remarked, that should even such a portion of water exist in the gas, it cannot be supposed that the acid should carry this with it into its saline combinations, and retain it so, that it should not be expelled by heat. It cannot be supposed to exist, therefore, in muriate of ammonia thus heated, and of course cannot account for the water obtained by the action of the metals on this salt.

When it is proved, that no extrinsic water exists in muriatic acid gas, there remain apparently only two modes on which the production of water can be explained,—either, that the metal may require less oxygen than is supposed in combining with the acid, so that a portion of water will remain undecomposed, to be deposited: or, that the oxide attracts more real acid, so as to liberate a larger proportion of water. The first of these suppositions is improbable, from the consideration of the law which regulates the combination of metallic oxides with acids,—that the quantity of acid is proportional to the quantity of oxygen, so that if an oxide were formed in these cases, at a lower degree of oxidation, it would only combine with a proportionally smaller quantity of acid, and the quantity of water detached from the combination would be the same.

No improbability is attached to the second supposition; and it has even some support from the consideration, that many metallic saline compounds, form with an excess of acid, and that it is difficult, with regard to a number of them, to procure them neutral. Metallic muriates, with excess of acid, seem in particular to be established with facility. And although an excess of metal be present in the action exerted on muriatic

acid gas, this may not prevent the formation of a super-muriate, more especially as the excess is in the metallic form, and exerts no direct action, therefore, on the real acid.

To ascertain if a super-muriate were formed in these cases, the product obtained from the action of the muriatic acid on the metal was raised to a heat as high as could be applied without volatilization, so that no loosely adhering acid might remain, and the air in the retort was repeatedly drawn out by a caoutchouc bottle. The solution from the residue both of iron and zinc was very sensibly acid. Some fallacy, however, attends this, from the circumstance, that the liquid state is necessary to admit of the indications of acidity, and in adding water to produce this, a change occurs in the state of combination, in a number of the metallic muriates; a super-muriate being formed, which remains in solution, and a sub-muriate being precipitated, so that the acidity of the entire compound cannot justly be inferred from that of the solution. I found, accordingly, that on adding water to the product from the action of the acid gas on zinc, this change occurs; a little of a white precipitate being thrown down, while the liquor remained acid. But the fallacy can be obviated, by adding only as much water as produces fluidity, without subverting the combination. Portions, therefore, of the residue were exposed to a humid atmosphere, until by deliquescence, liquors were formed transparent, without any precipitation; and these were strongly acid, reddening litmus paper when it was perfectly dry and warm. I farther found, that the product of the solution of zinc in liquid muriatic acid, when digested with an excess of metal, and evaporated to dryness, afforded by deliquescence a liquor sensibly acid. And in both cases, even when the solid product was retained liquid by heat, acidity was indicated by litmus paper. Lastly, What is still less liable to objection, the residue

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in the experiment of heating the muriate of ammonia with the different metals, afforded similar indications of acidity.

These results appear to establish the production of a supermuriate in the action of these metals on the acid, and this accounts for the appearance of a portion of water, since, supposing water to exist in muriatic acid gas, the quantity combined with that proportion of acid which would establish a neutral compound, is the quantity required to oxidate the metal to form that compound; and if any additional portion of acid enter into union, the water of this must be liberated, or be at least capable of being expelled.

It was of importance, in relation to this question, to ascertain the quantity of hydrogen obtained from a given quantity of muriatic acid gas; for, if the whole water essential to the acid is decomposed by the action of the metal, half the volume of hydrogen ought to be obtained,—muriatic acid gas being composed of equal volumes of oxymuriatic gas and hydrogen gas. I made this repeatedly the subject of experiment, by heating zinc and iron in muriatic acid gas. There are difficulties in determining the proportion with perfect precision; but the quantity of hydrogen always appeared to be less than the half; and on an average, about twelve measures were obtained, when thirty measures of the other had been consumed, a result conformable to the liberation of a portion of the combined water of the gas.

Whether the production of water in these experiments is satisfactorily accounted for, on the cause now assigned, may be subject of farther investigation. In the sequel, I shall have to notice another principle, on which perhaps it may fall to be explained. Whether accounted for or not, it is obvious, that the fact itself is not invalidated by the theoretical difficulty; and also, that in relation to the argument with regard to the na-

ture of muriatic and oxymuriatic acids, it remains equally conclusive. In the doctrine of the undecomposed nature of chlorine, muriatic acid gas contains neither water nor oxygen, and the metal employed certainly contains none. These are the only substances brought into action, and it is impossible that water should be a product of their operation. On the opposite doctrine, water is held to exist in muriatic acid gas to the amount of one-fourth of its weight; and it is conceivable, that by some exertion of affinities, a portion of it may be liberated. If we were unable to explain the *modus operandi*, this would remain a difficulty no doubt, but not, as in the opposite system, an impossible result.

It is to be admitted, indeed, that in none of these cases, is the entire quantity of water which must be supposed to exist in muriatic acid gas obtained; and so far the proof is deficient. But neither from the nature of the experiments is this to be looked for; and I give more weight to the argument, from having always found certain portions of water to be procured, while, on the opposite doctrine, there should be none. In those cases where supposing water to be present in muriatic acid gas, it ought to be obtained in the full quantity, it uniformly is so, though the proof from these is rendered ambiguous, by the result being capable of being explained on a different hypothesis.

PART

PART II.

OBSERVATIONS ON THE CHEMICAL CONSTITUTION OF MURIATIC ACID GAS, AND ON SOME OTHER SUBJECTS OF CHEMICAL THEORY.

Admitting water to be procured from muriatic acid gas in those forms of experiment, direct or indirect, in which the agency of no other substance that can afford it, is introduced, the conclusion seems necessarily to follow, which forms the basis of one of the two systems under which the relations of oxymuriatic and muriatic acids have of late years been explained, —that oxymuriatic acid is a compound of muriatic acid with oxygen; and that muriatic acid in its gaseous state, contains combined water. This doctrine, accordingly, may be maintained, and may even perhaps be just. It is not, therefore, from the consideration of any deficiency in its support, that I depart from it in the following observations, but that I consider the view I have to propose as perhaps more probable, or at least as, on the whole, according better with the present state of chemical theory. In a science such as Chemistry, the principles of which rest, rather on probable evidence, than on demonstration, it is of importance to present a subject in every point of view under which it may be surveyed; and this must serve as an apology for the speculations I have now to offer.

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There are, I believe, only two arguments to which any weight is due in support of the opinion that chlorine is a simple substance, which by combination with hydrogen forms muriatic acid. One is drawn from the analogy resting on the general fact, sufficiently established, that acidity is in different cases the result of the agency of hydrogen; the other, from the analogy in the chemical relations of chlorine and iodine.

Sulphur forms with hydrogen a compound unequivocally acid. The compound radical of prussic acid Cyanogen, discovered by the able researches of Gay Lussac, likewise acquires acidity when it receives hydrogen. Acidity, therefore, is a property not exclusively connected with oxygen; it is also communicated by hydrogen; and when chlorine with hydrogen gas, forms muriatic acid gas, the agency exerted may be considered as similar to that arising in other cases, of the production of an acid from the action of hydrogen.

This is confirmed by the relations of iodine. It, too, forms an acid by combination with hydrogen; and the chemical agencies of iodine are in several other respects similar to those of chlorine. When the one, therefore, is considered as a simple body, (and there is no absolute proof that iodine is a compound,) the other is, with probability, placed in the same class. And certain analogies existing between sulphur and iodine, serve to connect and confirm these views. Each of them forms an acid with hydrogen; each of them also forms an acid with oxygen. But chlorine exhibits precisely the same points of resemblance: with hydrogen, it forms muriatic acid; with oxygen, it forms chloric acid. Its chemical relations, with regard to acidity, being thus similar, seem to require the same explanation to account for them.

These facts lead undoubtedly to views of chemical theory, different from those which had before been established; and on which

which the old doctrine with regard to the nature of muriatic and oxymuriatic acids rests. It may be well, therefore, to inquire how far they may modify the conclusions to be drawn, admitting even that oxymuriatic acid contains oxygen, and that muriatic acid gas affords water.

When water is obtained from muriatic acid gas, it does not necessarily follow, that it has pre-existed in the state of water. It is equally possible, *a priori*, that its elements may be present in simultaneous combination with the acid, or its radical, —that the acid is a ternary compound of a radical with oxygen and hydrogen; and that it is decomposed in those processes by which water is procured, the hydrogen, with the requisite proportion of oxygen, combining to form water; and its radical, with any excess of oxygen, remaining in union with the substance by which the change has been effected.

If this view were adopted with regard to muriatic acid, the same view might, on the same grounds, be applied to the other acids which appear to contain water in intimate combination, and in a definite proportion. And such an acid, the radical and precise constitution of which are known, may be best adapted to illustrate the hypothesis.

Sulphuric acid affords water when it is submitted to the action of an alkaline base; and the quantity of this water appears to be definite, amounting to 18.5 in 100 of the strongest acid which can be procured in an insulated state; 100 parts of this acid, therefore, are considered as composed of 81.5 of real acid, (consisting of 32.6 of sulphur, and 48.9 of oxygen,) with 18.5 of water. But if, instead of this view of its constitution, it be considered as a ternary compound of sulphur, oxygen, and hydrogen, its composition will be 32.6 of sulphur, 65.2 of oxygen, and 2.2 of hydrogen. In those processes by which water is obtained from it;—in the action, for example, of an alkaline

base,

base, and subsequent exposure to heat, the composition is subverted by the affinities exerted; the hydrogen unites with the requisite proportion of oxygen, forming water, and the remaining oxygen with the sulphur unite with the base. In the action of a metal on the acid, there is the same result; only by the attraction of the metal to oxygen, the whole of that element is retained, and the hydrogen is disengaged.

Muriatic acid gas, then, according to this doctrine, is the real acid, a ternary compound of a radical (at present unknown) with oxygen and hydrogen, exactly as sulphuric acid in its highest state of concentration, is the real acid, a ternary compound of sulphur, oxygen, and hydrogen. When it is submitted to an alkaline base, the action exerted causes its decomposition; its hydrogen, and part of its oxygen, combine to form water, and its radical, with its remaining oxygen, unite with the base, forming a neutral compound, analogous to what other acids of similar constitution form. When a similar result is obtained from the action of a metal, its whole oxygen must be considered as retained, and its hydrogen is liberated.

Nitric acid in its highest state of concentration, is not a definite compound of real acid with about a fourth of its weight of water; but a ternary compound of nitrogen, oxygen, and hydrogen. Phosphoric acid is a triple compound of phosphorus, oxygen, and hydrogen; and phosphorous acid is the proper binary compound of phosphorus and oxygen. The oxalic, tartaric, and other vegetable acids, are admitted to be ternary compounds of carbon, oxygen, and hydrogen, and are therefore in strict conformity to the doctrine now illustrated.

A relation of the elements of bodies to acidity is thus discovered, different from what has hitherto been proposed. When a series of compounds exists, which have certain common

mon characteristic properties, and when these compounds all contain a common element, we conclude with justice, that these properties are derived more peculiarly from the action of this element. On this ground LAVOISIER inferred, by an ample induction, that oxygen is a principle of acidity. BERTHOLLET brought into view the conclusion, that it is not exclusively so, from the examples of prussic acid and sulphuretted hydrogen. In the latter, acidity appeared to be produced by the action of hydrogen. The discovery by GAY LUSSAC, of the compound radical cyanogen, and its conversion into prussic acid by the addition of hydrogen, confirmed this conclusion ; and the discovery of the relations of iodine still farther established it. And now, if the preceding views are just, the system must be still farther modified. While each of these conclusions is just to a certain extent, each of them requires to be limited in some of the cases to which they are applied; and while acidity is sometimes exclusively connected with oxygen, sometimes with hydrogen, the principle must also be admitted, that it is more frequently the result of their combined operation.

There appears even sufficient reason to infer, that from the united action of these elements, a higher degree of acidity is acquired than from the action of either alone. Sulphur affords a striking example of this. With hydrogen it forms a weak acid. With oxygen it also forms an acid, which, though of superior energy, still does not display much power. With hydrogen and oxygen it seems to receive the acidifying influence of both, and its acidity is proportionally exalted.

Nitrogen with hydrogen forms a compound altogether destitute of acidity, and possessed even of qualities the reverse. With oxygen in two definite proportions, it forms oxides; and it is doubtful, if in any proportion, it can establish with oxygen an insulated acid. But with oxygen and hydrogen in

 union

union it forms nitric acid, a compound more permanent, and of energetic action.

Carbon with hydrogen forms compounds which retain inflammability without any acid quality; with oxygen it forms first an inflammable oxide, and with a larger proportion a weak acid. But, combined with both hydrogen and oxygen, in different proportions, it forms in the vegetable acids compounds having a high acidity. These acids, therefore, are not to be regarded, according to the theory of LAVOISIER, as composed of a compound base of carbon and hydrogen, acidified by oxygen, but of a simple base, carbon, acidified by the joint action of oxygen and hydrogen.

Muriatic acid itself presents the same result. Oxymuriatic acid must be considered, according to this doctrine, as a compound of an unknown radical, (*Murion*, if the term may be allowed), with oxygen, analogous in this respect to sulphurous acid, except that in the latter there is an excess of base, in the former an excess of oxygen: And oxymuriatic acid, with the addition of hydrogen, forms the ternary compound muriatic acid, as sulphurous acid with the same addition forms hydro-sulphuric acid, with a deposition of the excess of sulphur. There is, accordingly, the strictest analogy between muriatic acid and those other acids, the sulphuric, nitric, &c. which contain both oxygen and hydrogen; while there is none, as BERZELIUS remarked, between it and those, such as the prussic acid or sulphuretted hydrogen, which contain merely hydrogen. This principle solves the difficulty which has always presented itself in the relation of muriatic and oxymuriatic acids on LAVOISIER's theory of acidity,—that the latter, though it has received an addition of oxygen, is inferior in acid power to the former. It is so precisely as the binary sulphurous acid is one of less energy of action than the ternary hydro-sulphuric acid, or as the carbonic is less powerful than the oxalic acid. The proper

per analogy is that of the oxymuriatic with the sulphurous acid, and the muriatic with the sulphuric; and under this point of view there is no anomaly, but strict conformity. And thus also is accounted for, what is at variance with the hypothesis of GAY LUSSAC, the total want of analogy between chlorine and sulphur, which he classes together, except in the single circumstance of acidity being communicated to both by hydrogen; while there exists a close analogy between sulphurous acid and oxymuriatic acid, in their most essential properties,—their gaseous form, their specific gravity, their suffocating odour, their power of destroying vegetable colours, their solubility in water; their remaining combined with it in congelation; their acidity, their combining weights, and their being attracted to the positive pole of the voltaic series; and any deviation from this analogy evidently arises from the excess of oxyen in oxymuriatic acid *.

It is obvious, that it would be in vain to seek for the discovery of real muriatic acid in its insulated form. It exists no more than real sulphuric or real nitric acid. The oxygen and sulphur, or oxygen and nitrogen in union with a salifiable base in the sulphates and nitrates, may not be in direct combina-

 tion,

* It is curious with regard to the most important of these analogies, that of the equivalent or combining weights, that oxymuriatic acid stands next to sulphurous acid; the former in Dr WOLLASTON's scale being 44, while the latter will be found to be 40. The acidity of oxymuriatic acid is fully established by the most unequivocal acid property, that of combining with alkalis, and forming neutral compounds. The saline nature of these compounds had been shewn by BERTHOLLET; that with lime has been demonstrated by Mr DALTON, who also pointed out the probability from the results, by double decomposition, that the acid combines in a similar manner with other salifiable bases; and the existence of these compounds has been established by Mr WILSON.

tion, nor capable of existing as a separate binary compound. The insulated binary compound of the radical of muriatic acid with oxygen is oxymuriatic acid, as the binary compound of sulphur and oxygen is sulphurous acid, and of nitrogen and oxygen, nitrous and nitric oxides.

Iodine, the discovery of which and its relations, has for a time given predominance to the new doctrine of chlorine, conforms sufficiently to these views. Some have considered it as a body belonging to the same class as chlorine; others regard it as more analogous to sulphur. It has little analogy to either, except in the property of forming acids with oxygen and with hydrogen. It differs remarkably from chlorine in its comparative inertness, its solidity, specific gravity, and great weight of its equivalent quantity. And it differs from sulphur in its want of inflammability, its solubility in water, and its being attracted to the positive pole of the voltaic series. All these analogies are preserved, and its relations connected, by considering it as an oxide, which, both from its specific gravity, the colour of its compounds, and the great weight of its equivalent quantity, has probably a metallic base; and which acquires acidity by an addition of hydrogen on the one hand, and on the other by the addition of oxygen, or of oxygen and hydrogen. In these respects, and in many of its chemical properties and relations, a considerable analogy exists between it and oxide of arsenic or oxide of tellurium. Or if it were to be classed as a simple substance, (on the ground of its not having been decomposed,)—which forms an acid with hydrogen, and another with oxygen and hydrogen; it does not in these respects offer any deviation compared with other acidifiable bases, or afford an argument of much weight in support of the undecomposed nature of chlorine.

The

The doctrine I have illustrated, affords a satisfactory explanation of the properties of the compounds formed by oxymuriatic acid with certain inflammables, particularly with sulphur and phosphorus. These undoubtedly present an anomaly in the other views that have been given of their constitution. In the old doctrine, they are considered as compounds of two real acids;—one of muriatic, with phosphorous or phosphoric acid; the other of muriatic, with sulphurous or sulphuric acid. But they have none of the properties which would be looked for in such a combination; they have no acidity, or if any appear in one of the compounds with phosphorus, it is to a very limited and doubtful extent; and they are substances even which have little energy of chemical action. In the new doctrine they are considered as compounds of chlorine with their bases, sulphur and phosphorus. Of course, as these bases form powerful acids with oxygen, and as chlorine is considered as an element of similar agency as oxygen, communicating similar powers, and conferring acidity even on hydrogen, they might, with not less reason than on the other doctrine, be expected to be acids of the greatest strength. The view I have stated accounts for their characters. They are ternary compounds, of the radical of muriatic acid with the particular inflammable,—sulphur, or phosphorus, with oxygen. The oxygen is not in sufficient quantity to communicate acidity, or, in one of the combinations of phosphorus, does so only to a very slight extent. But when water is added, a sufficient proportion of oxygen is supplied to produce this result, and the acidity is exalted by the corresponding hydrogen entering into the combination. What has been called Phosgene Gas, procured under certain circumstances from the action of oxymuriatic gas and carbonic oxide, may be regarded as of a similar nature, the agency of a small portion of water or of hydrogen being probably essential to its formation,

mation, a circumstance which serves to account for the discordant results with regard to its production *.

It deserves remark, that while there runs through the whole series of acidifiable bases in relation to their combinations with oxygen and hydrogen, a general analogy, there is also some deviation, and something with regard to each that is specific. Sulphur affords the most perfect example of their agency. It forms an acid with hydrogen; it forms another with oxygen; and a third, still more powerful, from the joint action of oxygen and hydrogen. Carbon forms an acid with oxygen; it also forms a series of acids of greater strength with oxygen and hydrogen; it acquires no acidity, however, from hydrogen alone; and with an inferior proportion of oxygen it forms an oxide. Phosphorus bears a strict analogy to sulphur, except that its combination with hydrogen does not give rise to acidity, a circumstance in which it resembles carbon. Nitrogen is peculiar in forming two oxides with different definite proportions of oxygen; it is doubtful if it forms a free acid with oxygen alone; but it conforms to the general law, and forms a powerful acid with oxygen and hydrogen. Assuming the existence of a simple radical of muriatic acid, it resembles sulphur, phosphorus and carbon, in forming an acid with oxygen, and one still more

* The difficulty of entirely excluding water and hydrogen from the constituents of this gas is sufficiently apparent. And the fact, that it cannot be formed from them by the action of the electric spark, but only by the continued action of solar light, is favourable to the above opinion. The conversion of carbonic oxide into carbonic acid, by the joint action of oxymuriatic gas and hydrogen, an experiment which I performed when the new hypothesis with regard to the nature of chlorine was brought forward, and which was attempted to be invalidated by some singular controversial methods, I consider as depending probably on the same principle.

more powerful with oxygen and hydrogen; but it differs in the peculiarity, that the proportion of oxygen to the base in the binary combination is considerably larger than in the ternary, so that the addition of hydrogen converts the one into the other; and also in its combining apparently with more numerous proportions of oxygen than any of the other acidifiable bases,—two circumstances which, as well as the difficulty of effecting its decomposition, probably depend on the same cause, the strength of its attraction to oxygen. The fluoric are similar to the muriatic compounds, except that the binary compound of the radical with oxygen cannot be obtained in an insulated form, and that its combinations with oxygen are less numerous. The relations of iodine or its radical are similar to those of the radical of muriatic acid, or perhaps rather to sulphur, except that its binary compound with oxygen does not appear to have acidity, in which it approaches to the metals. The metals usually combine with oxygen so as to form oxides; some of them also form acids with oxygen, or with oxygen and hydrogen; and these last usually also combine with hydrogen alone. This fact, of some of the metals forming acids, is so far an anomaly, since their compounds with oxygen rather form alkalis, and no other substances give rise to both results; the greater number of the substances, too, which form acids with oxygen or hydrogen, are evidently, from the smallness of their combining quantities, not of a metallic nature. Still the connection between the two classes is in some measure established on the one hand, by nitrogen, which with hydrogen forms an alkali, and on the other by iodine, which has properties and relations common to both.

In some cases it is probable, that there is a variation in the proportions of these ternary combinations, giving rise to a diversity of products, which exist only in combination with those

bodies

bodies by which their formation is determined, and, being modified by any process causing their evolution, are not easily observed. It is doubtful if the same base in any case forms different acids by combination with oxygen in different proportions, or by combination with hydrogen in different proportions. But the example of the vegetable acids seems to shew that this may occur in the united action of oxygen and hydrogen; carbon acidified by different proportions of these elements, constituting the composition of these acids. Other bases may present similar results. The radical of muriatic acid may unite with other proportions of oxygen and hydrogen than those which form muriatic acid; and this might afford a solution of the theoretical difficulty of the production of water in the experiments in the first part of this memoir, independent of the explanation of it from the formation of a super-muriate. A compound may be formed with less oxygen and hydrogen than what exist in muriatic acid, in combination with the metal acted on, and thus a portion of water may be liberated. Nor will it be easy to establish this by any difference in the product, as it can scarcely be submitted to any examination, but by processes which change the result. The chloric acid which, according to GAY LUSSAC, cannot exist insulated without water, may be in like manner a ternary compound of these elements in other proportions. Prosecuting the same analogy, the glacial or fuming oil of vitriol may be, not what has lately been asserted, real sulphuric acid, (for probably no such substance as that to which this term has been applied, can be obtained insulated), but a compound of sulphur with oxygen and hydrogen, in proportions different from those which constitute common oil of vitriol. Nitrous acid, if it cannot be formed without water, may be a compound of nitrogen with a smaller proportion

proportion of oxygen and hydrogen, than nitric acid. And some of the acids lately described, of which phosphorus is the base, may arise from variations of proportions of this kind.

The view which I have now illustrated, I must add, is not to be regarded as mere speculation. The evidence in support of it, is just as conclusive as that from which the opposite opinion is inferred. The obtaining water from a compound is no necessary proof that water pre-existed in it; and conversely, the causing water to enter into combination in a compound, is no necessary proof that it remains in the state of water in the product. In many cases we draw the reverse conclusions, considering water as being formed where it is obtained, and as decomposed where it is communicated. And in the case of its relation to acids, it will be found that there is no strict evidence of its existing as water in combination with what is considered as the real acid; and of course the conclusion is equally open to be drawn, that it exists in these combinations in the state of its elements, and that when obtained, it is a product of a change of composition.

It is even more probable, *a priori*, that the ultimate elements should act on each other where energetic affinities are evidently exerted, than the immediate principles, and the relations of these elements will determine the combinations, and the proportions. And by admitting this view, we avoid the anomaly which is presented in ascribing to the agency of water effects so different from those to which it usually gives rise. In general, water operates on bodies simply as a solvent, overcoming cohesion in solids, diluting liquids, or absorbing gases, without otherwise modifying their properties, or communicating to them any important chemical powers. But in the particular cases now referred to, it is supposed to produce the effects of

the most energetic chemical agent; it enters into combination in proportions strictly definite, is retained by the most powerful affinities; communicates new and characteristic properties; and is essential even to the existence of these compounds, in an insulated form. Berzelius and Gay Lussac have stated, that it is to be considered as a base necessary to retain the elements of the acid combined, though without neutralising the acid properties,—an opinion which in itself, and still more with this condition, is certainly sufficiently incongruous. And both theories admit equally of incongruity in the supposed presence and energetic action of water in acids. The old doctrine admits its influence in sulphuric, nitric, phosphoric, and muriatic acids, though at variance with its principle, that oxygen is the element which confers acidity, or at least having no conformity to that principle, nor receiving explanation from it. The new doctrine refuses to admit it with regard to muriatic acid, but admits it in all the others,—an exception which serves only to render the system more objectionable by the violation of analogy; while the admission with regard to the others is equally incapable of being accounted for on any principle it affords. By considering oxygen and hydrogen as elements conferring acidity, a satisfactory solution is afforded of the effects produced in these cases by their joint operation; and independent of this, it is much more probable, *a priori*, that such effects should arise from the action of elements so powerful, than from the agency of water, which, in its general relations, exerts such feeble powers. Lastly, The principle on which the presence of combined water in these acids has been supposed to depend,—that of the strong attraction of the acid to water, seems altogether fallacious; for on this principle sulphurous acid should also contain combined water, and sulphuretted hydrogen, and even carbonic acid, might be expected to retain a small

small portion. The whole evidently depends on difference of constitution. Sulphurous acid, sulphuretted hydrogen, and carbonic acid, are binary compounds, and therefore yield no water, nor retain any in intimate combination; and in the others, the proportion of water supposed to exist will be found to have no relation to the attraction of the acid to water, so far as this can be inferred, as is evident from the example of phosphoric acid affording as much as sulphuric or nitric; but to the relations of its elements, and more particularly of its oxygen, to the radical. This last fact affords nearly a demonstration, that the constitution is that of simultaneous combination of the elements, and not that of water and acid.

That water may also exist in immediate combination with acids, without being resolved into its elements, is sufficiently possible; and it probably is in this state, in those cases, in which there are no indications of an intimate combination, or definite proportion. It may then be considered as in solution similar to that in which it holds salts dissolved, or, what is a closer analogy, similar to that in which it holds dissolved the vegetable acids, which are admitted to be ternary compounds of carbon, hydrogen, and oxygen. The opposite view applies only to that portion of water considered as essential to the body in an insulated state, and in which it is combined in a definite proportion, observing in its relations, or the relations of its elements, equivalent proportions to other bodies.

In the last place, Considering this opinion in relation to the two opposite views which have been maintained with regard to the constitution of oxymuriatic and muriatic acids, while it has all the evidence in its favour from which the existence of water in muriatic acid gas is inferred, and all the analogies by which this is confirmed; it has the support which the doctrine of the undecompounded nature of chlorine derives from the re-

 lations

lations of sulphur, iodine, and cyanogen; and from the induction that hydrogen, as well as oxygen, communicates acidity. It avoids, at the same time, the improbability which attends that doctrine, in its leading principle, that muriatic acid contains no combined water, though other powerful acids are held to contain it, and though it affords water by the very same processes by which they yield it; and in the still greater violation of analogy, (the most extraordinary perhaps ever admitted in chemical reasoning), involved in the conclusion, that the compounds which this acid forms with salifiable bases, though the same in all generic properties with those formed by other acids, are not of similar constitution, and are not even of a saline nature. It unites the advantages, therefore, of both doctrines, and connects under one system facts which are otherwise insulated, and partial generalisations, which, instead of having any relation, seem opposed to each other.

The same general view, I have still to add, may be farther extended. Alkalinity, as well as acidity, is the result apparently of the action of oxygen; the fixed alkalis, the earths, and the metallic oxides, which all contain it as a common element, forming a series in which it is difficult to draw any well defined line of distinction. Ammonia alone remains an exception: it contains no oxygen, and yet possesses in a very marked degree all the alkaline properties,—an anomaly so great, as to have led almost every chemist to infer that oxygen must exist as an element in one or other of its constituent principles; and as nitrogen is the one apparently least elementary, it has been supposed to be a compound containing oxygen. The result may be accounted for, however, on a very different principle. As hydrogen, in some cases, give rise, as well as oxygen does, to acidity, so it may in other cases give rise to alkalinity. Under this point of view, ammonia is a compound of which

which nitrogen is the base, deriving its alkaline power from hydrogen; it stands, therefore, in the same relation to the other alkalis, that sulphuretted hydrogen does to the acids. And thus the whole speculation with regard to the imaginary metallic base Ammonium, and the existence of oxygen in ammonia, and in nitrogen, falls to the ground, while the anomaly presented by this alkali is removed. If the claim of the lately discovered principle in opium, *Morphia* as it has been named, to the distinction of an alkali be established, as from its origin it must probably have a compound base, it may, if it contain hydrogen, bear the same relation to the other alkalis, that prussic acid does to the acids; or if it contain oxygen, it will be analogous to the vegetable acids.

The fixed alkalis, and the alkaline earths, are considered as containing water in intimate combination, in a definite proportion; and it is doubtful if they can be obtained free from it in an insulated state, retaining at the same time their alkaline properties. It is obvious, however, that the elements of water may exist in combination with the base: that potash, for example, is not a compound of an oxide of potassium with water, but of potassium, oxygen and hydrogen. Hence when, on adding water to peroxide of potassium, potash is produced, and oxyen gas is disengaged; this is not owing, as has been supposed, to the excess of oxygen in the peroxide being expelled, and the water taking its place; but to the water being decomposed, and a portion of its hydrogen entering into the combination, to form the alkali, while the corresponding oxygen is liberated. If hydrogen were brought to act on peroxide of potassium, the alkali would in like manner be formed. With the peroxide of barium, this very change, from the action of hydrogen, takes place; the hydrogen, according to the usual explanation, combining with its oxygen, and forming water, which

unites

unites with the real earth, forming the hydrate;—in other words, and according to the strict expression of the fact, the hydrogen entering into the composition, and forming the barytes;—a result perfectly analogous to the formation of muriatic acid from oxymuriatic gas by the agency of hydrogen.

The evidence in support of this doctrine, it is evident, is of the same kind as that with regard to the doctrine applied to the acids. There is the same superior probability in favour of the conclusion, that the elements of water, rather than water itself, exist in these compounds, from the consideration, that modifications of properties so important, are more likely to arise from the agency of these elements, than from any action which water can exert. And that water does not exist in them, in consequence of the strength of attraction which the real alkali, as it has been considered, exerts towards it, is evident from this, that on the same principle ammonia ought to contain combined water in its insulated form, which is not the case. The combination of water, therefore, or rather of its principles, in these compounds, depends on relations subsisting among the ultimate elements, not on an affinity exerted by the alkali itself; and this adds confirmation to the conclusion, that these elements are in ternary union.

Their superior alkaline energy, compared with the common metallic oxides, may obviously arise from the joint action of the hydrogen and oxygen, in the same manner that the acidity of the ternary, compared with the binary acids, is increased by a similar constitution. Thus the class of alkalis will exhibit the same relations as the class of acids. Some are compounds of a base with oxygen: such are the greater number of the metallic oxides, and several, probably, of the earths. Ammonia is a compound of a base with hydrogen. Potash, soda, barytes, strontites, and, probably, lime, are compounds of bases with oxygen

oxygen and hydrogen; and these last, like the analogous order among the acids possess the highest power. Many of the metallic oxides, however, in the state in which they combine with the greatest facility with the acids, are hydrates,—that is, supposed compounds of the oxide with water, but probably ternary compounds of the metal with oxygen and hydrogen; and their facility of combination, may depend on this constitution. The same principle explains the necessity, not otherwise easily accounted for, of the presence of water, to enable some of the earths, as barytes, to combine with acids.

There are two views under which the neutral salts may be considered in the preceding theory. It has been shown, that when water is obtained in the action of a salifiable base, whether alkali, earth, or metallic oxide, there is reason to infer that this water is formed by the hydrogen and part of the oxygen of the acid entering into binary combinations; and when water is obtained from an alkali by the action of an acid, there is the same reason to believe, that it is formed by the combination of the hydrogen of the alkali with a portion of its oxygen. In these cases it may be supposed, that the radical of the acid combines with its remaining oxygen, forming a binary compound, which may still be considered as an acid; and that the radical of the alkali combines with its remaining oxygen, forming a binary compound, which may be regarded as an alkali; and these two compounds may unite with each other, forming the neutral salt. This is conformable nearly to the common doctrine. But there is another point of view under which the subject may also be considered. A ternary combination, into which oxygen and hydrogen enter, gives rise apparently to a higher state of acidity, and to a greater degree of alkaline energy than is acquired from a mere binary combination into which oxygen enters. It is doubtful, therefore, if such binary compounds

pounds were formed, if they would constitute either acid or alkali. And there is at least no proof of their formation. In all these cases, while the hydrogen present combines with the requisite proportion of oxygen forming water, the radical of the acid, and the radical of the base, may enter into union with the remaining oxygen, and form a ternary compound. And where hydrogen is not present, such a combination may be at once established.

It is not easy to determine which of these opinions is just. The reason above stated, renders the latter, perhaps, more probable; and the view which leads to the conclusion, that in the constitution of the acids and alkalis, the three elements, when present, are in simultaneous combination, leads also to a similar conclusion with regard to the constitution of the neutral salts. If this be adopted, neutralisation is not the saturation of acid with alkali, and the subversion of the properties of the one by the opposed action of those of the other; but is the change of composition of both, and the quiescence of the elements, in that proportion in which their affinities are in a state of equilibrium without any excess. The compounds, therefore, have little activity; and energy of action is restored only by the reproduction of substances, which, by their mutual attractions, tend to the same state of quiescence.

All these results display more fully the extensive relations of the two elements, oxygen and hydrogen. They do not act merely in opposition, as had been imagined, but more frequently in union, producing similar effects. Hydrogen is of nearly equal importance with Oxygen, and the principal details of chemistry, consist in their modified action on inflammable and metallic bodies.

XVIII.

XVII. *Experiments on the Relation between Muriatic Acid and Chlorine; to which is subjoined the Description of a New Instrument, for the Analysis of Gases by Explosion. By* ANDREW URE, M. D. *Professor of the Andersonian Institution, and Member of the Geological Society.*

(*Read Nov.* 17. 1817.)

PART I.

THE Chloridic Theory, though more limited in its application to chemical phenomena, than the Antiphlogistic, may justly be regarded as of scarcely inferior importance. If established, it leads to the adoption of entirely new views concerning combustion and many of its products; it removes the muriates, a set of apparently well characterised saline bodies, from the class of salts altogether; and it has given birth, by analogy, to two new genera of compounds, in which iodine and fluorine, like chlorine, act a corresponding part with oxygen, in the system of LAVOISIER.

This new era in chemical science, unquestionably originated from the masterly researches of Sir HUMPHRY DAVY on Oxymuriatic Acid Gas; a substance which, after resisting the most powerful means of decomposition which his sagacity could invent, or his ingenuity apply, he declared to be, according to the true logic of chemistry, an elementary body, and not a

compound of muriatic acid and oxygen, as was previously imagined, and as its name seemed to denote. He accordingly assigned to it the term Chlorine, descriptive of its colour; a name now generally used.

Chlorine when combined with an equal volume of hydrogen, forms Muriatic Acid Gas, the Hydrochloric of GAY LUSSAC. This muriatic acid gas, hygrometrically dry, unites with its own bulk of dry ammoniacal gas, to constitute the dry pulverulent solid called Sal Ammoniac. Hence this saline body is ultimately composed of chlorine and hydrogen, for its acid; and of azote and hydrogen, for its base. By comparing the weights of muriatic acid and ammoniacal gases, in equal volumes, we obtain the proportion of 67.8 muriatic acid gas, to 32.2 ammonia, for the composition of 100 parts by weight of the solid salt. If we saturate liquid muriatic acid with gaseous ammonia, or with the base of the ammoniacal carbonate, and evaporate carefully to dryness, we find the resulting salt to have precisely the same constitution, namely, in 100 parts, 51 of dry muriatic acid, equivalent to 67.8 of the acid gas, and the remainder 32.2 ammonia. This concurrence of results, whatever way the salt may be obtained, is fully demonstrated in my researches on the ammoniacal salts, (*Annals of Philosophy for September* 1817), and proves it to be a substance of very uniform and determinate composition.

Those chemists who consider chlorine to be oxymuriatic acid, must suppose, when a volume of it weighing 44.13 unites with an equal volume of hydrogen, weighing 1.32, that, in the resulting hydrochloric or muriatic acid gas $= 45.45$, this hydrogen exists combined with 10.00 of oxygen, its saturating quantity, forming 11.32 of constituent water. In this view, muriatic acid gas, like gaseous, sulphuric, and nitric acids, contains water as an essential element. There seems to be no violation

violation of chemical analogy in this supposition. The quantity will be represented by the fraction $\frac{11.32}{45.45}$, being nearly one-fourth.

If chlorine, however, be a simple body, which forms with hydrogen, muriatic acid gas, then sal ammoniac is rightly named Hydrochlorate of Ammonia. And since ammonia itself results from three volumes of hydrogen and one of azote, condensed into two volumes, that saline body can contain neither water, nor its indispensable element oxygen.

On the other hand, if chlorine be oxymuriatic acid, then the fourth part of water existing in the resulting muriatic acid gas, must necessarily enter into the sal ammoniac as an essential constituent; for the whole ponderable matter of that gas, as well as of the ammonia, passes into the salt. This water being as indispensable an ingredient of sal ammoniac as it is of oil of vitriol; heat alone can no more separate it from the former, than it can from the latter compound.

Moreover, if we decompose sal ammoniac by the agency of any body containing oxygen, an evident source of fallacy exists relative to the watery product, which may be referred by the supporters of the chloridic theory, not to the salt itself, but to the hydrogen of the hydrochloric acid, united with the oxygen of the decomposing substance. This ambiguous interpretation, is experimentally illustrated, in my paper on the Ammoniacal Salts.

If, however, we shall decompose that equivocal salt, by means of a substance, which certainly contains no oxygen; and if we still obtain water in nearly the above proportions; then this result is no longer equivocal, nor will it admit of two interpretations. We must thenceforth be compelled to recognise in muriatic acid gas, as in the other acid vapours, WATER as an ingre-

dient essential to its constitution; and to acknowledge that chlorine consists of a base united to oxygen, or is in fact oxygenated muriatic acid, as LAVOISIER and BERTHOLLET taught, and as the whole chemical world believed, till their faith was lately shaken or subverted, by the predominating genius of Sir HUMPHRY DAVY.

With the view of deciding the above important controversy, I performed the following experiments.

Of sal ammoniac, kept for some time in a platina capsule at a subliming heat, to remove every particle of adhering moisture, a known quantity was put into a glass tube, and made to slide down to the one end, which had been hermetically sealed. Over it a given weight of bright metallic laminæ, cut into slender segments, was lightly pressed. The salt occupied in general about one inch of the tube; the laminæ four or five inches. Silver, copper, and turnings of iron made with a dry tool, were employed in successive experiments. The open extremity of the tube was drawn out to a point, and recurved, so as to pass under a vessel inverted on the mercurial pneumatic trough. Between this and the portion containing the metal, there was a length of six or more inches of tube, which was kept cool. In one variation of the experiment, a tube of Reaumur's porcelain was used for containing the materials, to which was firmly luted by a collar of caoutchouc, a glass tube, with a little globe blown in its middle, and its loose end plunged, as usual, into the mercurial trough.

When tubes of crystal glass were employed, the part containing the materials was lodged in a semicylindrical case of iron, which traversed a small charcoal furnace, five or six inches in diameter. The metallic laminæ being raised to full ignition in day-light, the case and tube were slightly moved forward,

forward, in order to bring a little of the salt within the sphere of the heat. Great nicety was required in the advancement of the sealed extremity; for the glass tube being perfectly softened in its middle, too sudden volatilisation of the salt never failed, by inflating and bursting it, to spoil the experiment. This accident frequently happened. On the other hand, if the central part of the tube was exposed to merely a dull red, the experiment would not succeed with silver and copper. At this temperature they did not decompose the sal ammoniac. When, however, the above-mentioned precautions were observed, dew could be perceived to settle speedily on the cool portion of the tube. This dew became more and more visible as the sublimation advanced, till, finally collecting into distinct drops, it trickled down the sides in striæ, and formed a filament along the bottom. To obtain good results of this kind, four or five hours must be devoted to one experiment, in which 20 grains of salt, and from 60 to 100 of metal, are employed. More rapid transmission of the salts effects mere sublimation. Bubbles of gas come over, which, with silver and copper laminæ, are found to be a mixture of ammonia and hydrogen. In this case, the liquid condensed, is water of ammonia.

The metallic laminæ are evidently heavier than before their introduction; but the increase of their weight could not be exactly ascertained, because a portion of the silver or copper is impressed on the inner surface of the tube, giving it a very beautiful iridescent and metallic lustre, similar to the colours of the diamond beetle, viewed in a microscope. The silver laminæ, have for the most part exchanged their native brilliant white, for a dull-brown or greyish hue; and instead of being eminently tough and ductile, have become more brittle than any substance with which I am acquainted. The slightest touch

touch of the finger breaks them across. Digested in pure nitric acid somewhat dilute, the segments only partially dissolve, bits of muriate of silver, of their own shape, being left in the liquid.

The ignited copper turnings, after experiencing the action of sal ammoniac, are found to have lost also their original lustre, and have acquired a dull brown colour. Digested in water, a liquid muriate is obtained, which gives the characteristic brown precipitate with prussiate of potash.

The most considerable of my experiments with turnings was made with the tube of Reaumur's porcelain, which, as it contains no oxide of lead, is not liable to any ambiguity on this score, and being capable of sustaining a very high heat without fusion, permitted me to obtain very satisfactory results indeed.

Thirty grains of recently heated sal ammoniac being put down to the sealed end, 200 grains of bright turnings of very pure soft iron were introduced over it, so as to occupy six inches of the tube. The glass tube above described, was attached by the elastic gum collar. The part holding the iron being brought to bright ignition, the sealed end of the tube was advanced by degrees almost imperceptible. As soon as the salt began to exhale, moisture began to condense in the glass tube, though none ever appeared prior to heating the sal ammoniac. The evolution of gas was much more copious than in any of the experiments with the other metals. When allowed to escape through the quicksilver into the air, it exhibited the dense cloud, and had the odour of muriatic acid. Received into a tube over mercury, and then exposed to the action of water, $\frac{5}{100}$ parts of the volume were absorbed, which on trial were found to be pure muriatic acid. The remainder was a mixture of azote and hydrogen, in the proportions very nearly

nearly that are known to constitute ammonia. I analysed this mixed gas, by explosion with half its volume of pure oxygen, in a peculiar apparatus, which I shall describe in the sequel. On firing 100 measures with the electric spark, 76.2 disappeared, $\frac{2}{3}$ of which, $= 50.8$, are hydrogen. Before explosion, the hundred volumes consisted of $66\frac{2}{3}$ ammoniacal gaseous matter, $+ 33\frac{1}{3}$ oxygen. Of these $66\frac{2}{3}$ parts, 50.8, are hydrogen, and 15.86 azote; or in the 100, $76.2 + 23.8$. But, by GAY LUSSAC, 1 volume of azote unites with 3 volumes of hydrogen to form ammonia. Hence 23.8 measures of azote should have been accompanied with only 71.4 of hydrogen, instead of 76.2 actually obtained. This excess of hydrogen is due to the decomposition of a little of the watery product, in the formation of the muriate of iron. That muriate of iron is formed, is proved by many circumstances. First, the disappearance of the acid in the gaseous products. Sal ammoniac being decomposed with its ultimate gases, will consist of two measures of those constituting the alkali + one measure of the muriatic. Hence 100 volumes should contain $33\frac{1}{3}$ of this acid gas; but they actually contained only about 5. Therefore about 28 measures, which form the difference, were condensed with the iron. Secondly, the iron turnings had increased in weight; they deliquesced speedily on exposure to the atmosphere, and, digested in water, they yielded an acerb-tasted solution of muriate of iron, giving with prussiate of potash a copious blue precipitate.

The quantity of muriate produced in the experiment, will depend on the proportion of turnings which have been but moderately heated; for the ammonia, in its passage over the strongly ignited iron, may be conceived to separate the oxygen, and thus prevent the formation of muriate.

Water

Water impregnated with muriatic acid equal in weight to nearly one-sixth of the sal ammoniac decomposed, is uniformly obtained by the above process. Scarcely a particle of ammonia seems to escape entire decomposition. The evolved muriatic acid, amounting to $\frac{5}{100}$ of the whole gaseous products, must carry off with it a portion of its constituent water. Hence we ought to find a little less water here condensed, than, by my experiments on the ammoniacal salts above referred to, sal ammoniac, viewed as a muriate, is shewn to contain.

It seems evidently to follow, from this experimental detail, that chlorine is oxygenated muriatic acid. Since dry sal ammoniac consists of ammonia and muriatic acid gases, both hygrometrically dry; and since water is obtained in its decomposition by pure metals; this water must have existed in the gaseous acid; for all experiments concur in proving ammonia itself to contain nothing but azote and hydrogen. And, finally, since muriatic acid gas is a compound of chlorine and hydrogen, the water derived from the resulting muriatic acid, demonstrates the presence of oxygen in the chlorine, or, in other words, that it is really oxymuriatic acid *.

All the experimental phenomena hitherto adduced in the chloridic controversy, were susceptible of explanation on both the old and new doctrine. Thus, the hydrogen which remains after tin is subjected at a high temperature to muriatic acid gas,

* If the Chloridic Theory be still retained, then the production of water in the above circumstances can be ascribed only to the decomposition of azote into oxygen and hydrogen, as has been already indicated in my paper on the Ammoniacal Salts. It is possible that this alternative may eventually be found the true one; yet in the present state of our knowledge, such an inference would be illogical.

gas, could be regarded with DAVY, as resulting from a metallic analysis of hydrochloric acid; or it might be derived from the combined water of muriatic acid, of which the oxygen became fixed in the muriate of tin. When chlorine also at high heats was made to act on earths or common metallic oxides, the evolved oxygen, could be referred with equal probability either to the solid or to the gas.

And though we ignite by the strongest voltaic power, charcoal or other combustibles in chlorine, still we shall not be able to convert it into muriatic acid gas, for want of the essential constituent water; no more than we can, without the same water, obtain oil of vitriol. Present water to chlorine, then light alone will separate its oxygen, and leave muriatic acid. Such, indeed, is the affinity existing between the muriatic acid basis and water, that those muriates which of themselves resist decomposition at a red heat, when exposed at that temperature to the vapour of water, are speedily resolved into gaseous muriatic acid, and their peculiar bases.

By restoring the theory of LAVOISIER and BERTHOLLET, we get rid of those mysterious and almost incomprehensible transformations which a drop of water has been lately conceived to produce on some of the muriates. Dried sea-salt, for example, when viewed as a compound of chlorine and sodium, is no sooner moistened, than a portion of water resolves itself into oxygen and hydrogen, whence result soda and hydrochloric acid, and a solution of muriate of soda. Expel the drop of water, we have a chloride of sodium once more; and we may repeat this invisible change for an indefinite number of times by the addition or subtraction of a little moisture. Thus we must consider dry salt and moist salt to be bodies widely and essentially different, the former containing neither alkali nor

 acid,

acid, while the latter contains both. This supposition, which the chloridic theory compels us to make, must surely be reckoned somewhat violent.

Description of an Apparatus for the Analysis of Gaseous Matter by Explosion.

The analysis of combustible gases, and supporters of combustion, reciprocally, by explosion, with the electric spark, furnishes, when it can be applied, one of the speediest and most elegant methods of chemical research. The risk of failure to which the chemist is exposed, in operating with the simple tube, from the ejection of the mercury, and escape or introduction of air; or of injury from the bursting of the glass, by the forcible expansion of some gaseous mixtures, has given rise to several modifications of apparatus.

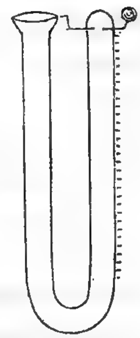
Volta's mechanism, which is employed very much at Paris, is complex and expensive *, while it is hardly applicable to experiments over mercury. Mr Pepys's ingenious contrivance, in which the glass-tube is connected with a metallic spring, to diminish the shock of explosion, is liable also to some of the above objections.

A very simple form of instrument occurred to me about two years ago, in which the atmospheric air, the most elastic and economical of all springs, is employed to receive and deaden the recoil. Having frequently used it since that time, I can now

* The price of the apparatus is about three guineas.

now recommend it to the chemical world, as possessing every requisite advantage of convenience, cheapness, safety and precision.

It is represented on the margin.



It consists of a glass syphon, having an interior diameter of from $\frac{2}{10}$ to $\frac{4}{10}$ of an inch. Its legs are of nearly equal length, each being from 6 to 9 inches long. The open extremity is slightly funnel-shaped; the other is hermetically sealed; and has inserted near it, by the blow-pipe, two platina wires. The outer end of the one wire is incurvated across, so as nearly to touch the edge of the aperture; that of the other is formed into a little hook, to allow a small spherical button to be attached to it, when the electrical spark is to be transmitted *. The two legs of the syphon are from $\frac{1}{4}$ to $\frac{1}{2}$ inch asunder.

The sealed leg is graduated, by introducing successively equal weights of mercury from a measure glass-tube. Seven ounces Troy and 66 grains, occupy the space of a cubic inch; and $34\frac{1}{4}$ grains represent $\frac{1}{100}$ part of that volume. The other leg may be graduated also, though this is not necessary. The instrument is then finished.

To use it, we first fill the whole syphon with mercury or water, which a little practice will render easy. We then introduce into the open leg, plunged into a pneumatic trough, any convenient quantity of the gases, from a glass-measure tube containing them previously mixed in determinate proportions. Applying a finger to the orifice, we next remove it from the trough in which it stands, like a simple tube; and by a little dexterity,

* In the figure, the ball should have been represented pendent from a hook.

dexterity, we transfer the gas into the sealed leg of the syphon. When we conceive enough to have been passed up, we remove the finger, and next bring the mercury to a level in both legs, either by the addition of a few drops, or by the displacement of a portion, by thrusting down into it a small cylinder of wood. We now ascertain, by careful inspection, the volume of included gas. Applying the fore-finger again to the orifice, so as also to touch the end of the platina wire, we then approach the pendent ball or button to the electrical machine, and transmit the spark. Even when the included gas is considerable in quantity, and of a strongly explosive power, we feel at the instant nothing but a slight push or pressure on the tip of the finger. After explosion, when condensation of volume ensues, the finger will feel pressed down to the orifice by the superincumbent atmosphere. On gradually sliding the finger to one side, and admitting the air, the mercurial column in the sealed leg will rise more or less above that in the other. We then pour in this liquid metal, till the equilibrium be again restored, when we read off as before, without any reduction, the true resulting volume of gas.

As we ought always to leave two inches or more of air between the finger and the mercury, this atmospheric column serves as a perfect recoil spring, enabling us to explode very large quantities without any inconvenience or danger. The manipulation is also, after a little practice, as easy as that of the single tube. But a peculiar advantage of this detachable instrument, is to enable us to keep our pneumatic troughs and electrical machine at any distance which convenience may require; even in different chambers, which, in the case of wet weather, or a damp apartment, may be found necessary to ensure electrical excitation. In the immediate vicinity of the water pneumatic cistern, we know how often the electric spark refuses

refuses to issue from a good electrophorus, or even little machine. Besides, no discharging rod or communicating-wire is here required. Holding the eudiometer in the left hand, we turn the handle of the machine, or lift the electrophorus plate with the right, and approaching the little ball, the explosion ensues. The electrician is well aware, that a spark so small as to excite no unpleasant feeling in the finger, is capable, when drawn off by a smooth ball, of inflaming combustible gas. Even this trifling circumstance may be obviated, by hanging on a slender wire, instead of applying the finger.

We may analyse the residual gaseous matter, by introducing either a liquid or a solid re-agent. We first fill the open leg nearly to the brim with quicksilver, and then place over it the substance whose action on the gas we wish to try. If liquid, it may be passed round into the sealed leg among the gas; but if solid, fused potash for example, the gas must be brought round into the open leg, its orifice having been previously closed with a cork or stopper. After a proper interval, the gas being transferred back into the graduated tube, the change of its volume may be accurately determined. With this eudiometer, and a small mercurial pneumatic cistern, we may perform pneumatic analyses on a very considerable scale.

It may be desirable in some cases, to have ready access to the graduated leg, in order to dry it speedily. This advantage is obtained, by closing the end of the syphon, not hermetically, but with a little brass cap screwed on, traversed vertically by a platina wire insulated in a bit of thermometer tube. After the apparatus has been charged with gas for explosion, we connect the spherical button with the top of the wire.

With the above instrument I have exploded half a cubic inch of hydrogen mixed with a quarter of a cubic inch of oxygen;

gen; as also, a bulk nearly equal of an olefiant gas explosive mixture, without any unpleasant concussion or noise; so completely does the air-chamber abate the expansive violence, as well as the loudness of the report. Projection of the mercury, or displacement of the gas, is obviously impossible.

PART

PART II.

(Read Jan. 19. 1818.*)*

EXPERIMENTS MADE WITH THE VIEW OF DETERMINING THE RELATION BETWEEN MURIATIC ACID AND CHLORINE.

To the inferences which I have ventured to draw, from the experimental results detailed in the preceding part of this paper, two objections may be offered.

First, That the aqueous product obtained in the decomposition of dry sal ammoniac, by ignited metallic laminæ, may possibly be derived from the azote of the ammonia, supposing azote not to be a distinct elementary substance, but a peculiar oxide of hydrogen. This notion occurred to me at an early period, in consequence of the anomalous, and unaccountable disappearance, of a portion of ammonia, and the concomitant production of water, as described in my experimental researches on the ammoniacal salts. To determine how far this view was correct in the present instance, I made the following experiment: Into a tube of green glass, sealed at one end, I put 30 gr. of desiccated sal ammoniac. Over it 200 gr. of pure iron turnings were placed, which occupied 5 inches of the tube. The open end of this was connected by a collar of caoutchouc, with a narrow tube of crystal glass, having a small sphere

sphere blown in its middle, and its other extremity plunged into pure water, in a glass pneumatic cistern.

When the part of the green glass tube where the turnings lay, was brought to ignition very visible in day-light, it was moved forward with its supporting semi-cylinder of iron, by imperceptible degrees. Thus the salt was exhaled from the bottom, so slowly, that its vapour, in traversing the numerous convolutions of the iron laminæ, was almost entirely decomposed into its constituent gases. The muriatic acid gas was (as formerly stated) partly condensed into muriate of iron, partly into liquid acid in the bulb, and partly into the water in the glass cistern.

Of permanent gaseous matter 106 cubic inches were collected, after the whole salt had been sublimed from the bottom of the tube. One hundred cubic inches of these were found by explosion with oxygen in my eudiometer, to consist of 77 hydrogen + 23 azote. Hence the total product of 106 cubic inches was composed of 81.62 hydrogen, and 24.38 azote.

Now 30 grains of sal ammoniac contain 9.66 gr. of ammonia. And since 18.178 gr. of this alkaline gas occupy the volume of 100 cubic inches, but are resolvable into double that volume, or 200 cubic inches of constituent gases; therefore 9.66 gr. of ammonia will give by their entire decomposition 106.28 cubic inches of gaseous products. Of these, one-fourth according to M. Gay Lussac's theory of volumes, is azote, and three-fourths hydrogen; or 26.57 of the former, and 79.71 of the latter.

Hence we see that the total bulk of evolved gases, coincides very nearly with the quantity known to exist in the ammonia. The azote, therefore, is not concerned in the production of the water. The deficiency of about two cubic inches of this gas, may be fairly ascribed to a small portion of the salt having escaped

escaped decomposition. The excess in the proportion of hydrogen, is due to the decomposition of the water of the muriatic acid, in the formation of the muriate of iron. The muriatic acid was precipitated from the dissolved and filtered muriate of iron, as well as from the liquid of the bulb, and of the trough, by nitrate of silver, and the muriate of silver corresponded very nearly with the quantity which 30 grains of sal ammoniac ought to afford. Of ignited red oxide of iron, treated with nitric acid, there was obtained 8.8 grains. These are equivalent, as we shall find in the sequel, to 19.06 gr. of the peculiar white muriate of iron formed in this process.

Having thus, I presume, given a satisfactory answer to the first objection, I shall proceed to the second. This may be urged by those who conceive that common sal ammoniac thoroughly desiccated by heat, and the salt resulting from the combination of the acid, and alkaline gases, in a dry state, are not identical in their composition.

Though the very numerous experiments which I had formerly made on the ammoniacal salts, had entirely satisfied me that well dried muriate of ammonia is uniform in its composition, in whatever way prepared; yet I deemed it a duty I owed to those who might entertain doubts on the subject, to make the analysis by ignited iron, of sal ammoniac formed from dry gaseous matter.

Accordingly, muriatic acid gas was evolved without heat from dry muriate of soda, and concentrated sulphuric acid; and ammoniacal gas, from a mixture of dry lime and dry sal ammoniac. Each gas being slowly generated, and slowly passed along a tube of thin glass, three feet long, surrounded with paper, kept moist with ether, was brought into contact with the other, in a small glass globe, furnished with two tubulures. Thirty-five grains of a brilliant white and very light pulveru-

lent matter, like magnesia, were obtained. These being heated for some time in a platina capsule, on a sand bath, to a temperature just under sublimation, lost exactly half a grain. The salt was found perfectly neutral by litmus paper.

Twenty grains of this hot and dry salt, were pressed down to the sealed end of a green glass tube; and 120 grains of pure iron turnings being put over them, the former arrangement of furnace, communicating tube, and mercurial trough, was adopted. After some hours of careful igneous decomposition, about three grains of liquid were obtained. A similar portion of muriatic acid gas, to what is formerly stated, was evolved, along with the gaseous elements of ammonia. Thus I conceive the second objection to be done away.

To place the identity of the two differently prepared salts, in the clearest light, I shall state one additional experiment. I took 5 gr. of that made by gaseous combination, and 5 gr. of the common kind, both well dried, and dissolving them in water, precipitated the whole of their acid, by nitrate of silver. The gently ignited muriate of silver, obtained from each, was almost exactly equal; the minute fractional difference, being an excess on the side of the ordinary sal ammoniac. Hence we see, that the latter contains as much acid, and consequently as little of the base or ammoniacal hydrate, (as we may now term it), as the former.

Doctor Murray, in the able critique which he has given, in his valuable System of Chemistry, on the chloridic hypothesis, adduces some experiments of his own, which he conceives establish the opposite, or oxymuriatic doctrine. They consisted in procuring by heat a visible portion of aqueous matter, from the saline compound of ammoniacal and muriatic acid

gases,

gases, both dried previous to their union. He heated the salt by itself, or passed it through hot charcoal.

I have exposed the above salt, as well as ordinary sal ammoniac, to very numerous and diversified trials of the same kind, but never could obtain in similar circumstances, any satisfactory product of water. Nor can I imagine on what principle this profound and ingenious chemist should have expected to obtain water from such sal ammoniac, by the agency of heat, or of heated charcoal. The water present in muriatic acid gas, by the old theory, which he espoused, is evidently combined, not hygrometric water; and ammoniacal gas certainly contains none of that liquid. Now, when the two dried gases unite to form a solid salt, their whole ponderable matter is condensed or fixed in it; and whatever existed in both these components, has become a permanent and essential constituent of the compound. One hundred cubic inches of muriatic acid gas, weighing 38.04 gr., unite to 100 cubic inches of ammoniacal gas, weighing 18.17 gr.; together affording exactly 56.21 gr. of sal ammoniac.

Now, whatever water existed in the acid gas, is indissolubly attached to the very existence of the salt, and will always sublime along with it, when heat is applied. Nay, though by a very intense heat, we resolve the solid into its ultimate elementary constituents, we shall recover nothing but hydrogen, azote, and muriatic acid gases, each as hygrometrically dry as before. It would, indeed, to my apprehension, be as reasonable to hope, to extract, by the agency of heat, the combined water of concentrated oil of vitriol. We shall only vaporize or distil; the water rising infallibly with the oxygen and sulphur; as it rises with the sal ammoniac, being essential to its first formation, and future existence. In both these instances, it is solely by fixing the acid with a base, in some salt, which

requires no water of composition, that we can ever expect to obtain the combined or latent water of sulphuric acid, or of muriate of ammonia. By this general principle, all my efforts have been directed.

I shall now endeavour to demonstrate, by particular experiments, the accuracy of these views.

1. Sal ammoniac was put down to the sealed end of a glass-tube; over it was placed a few inches of river sand, which, after having been digested with muriatic acid, had been well edulcorated with water, and ignited, in a platina crucible. On passing the vapour of the salt, through the ignited sand, liquid of a reddish-brown colour was copiously condensed in the projecting and cool part of the tube. This liquid was acidulous muriate of iron. I naturally ascribed the origin of this to the red oxide of iron, still contained in the sand after the above operations.

2. To verify this idea, I then took pounded flints, and obtained from the same quantity of salt, in various repetitions of the process, a very small quantity of blue liquid, which seemed to be an ammoniacal solution of copper, by its colour and smell.

3. I next had recourse to quartz nearly pure. The quantity of liquid obtained in the same circumstances, was now very inconsiderable indeed; and I conceived it might be ascribed to some interspersed particles of mica and felspar, whose alumina might fix a little of the dry acid, and leave water of ammonia.

4. When pure rock-crystal was employed, the aqueous product became almost evanescent. The salt sublimed through the ignited quartz powder, without any apparent decomposition.

5. When

5. When strongly calcined charcoal was employed, I obtained no traces of water at all. It will, moreover, be readily granted by every chemist, that, from the equivocal nature of common charcoal, as prepared with greater or less care, and from the uncertainty of expelling the whole gaseous matter, and moisture it so greedily imbibes, no important inference can be drawn from any results in which it is concerned *.

The traces of moisture which Dr MURRAY observed in his experiments, must have been the adhering or hygrometric water of the sal ammoniac. He has indeed assigned some hypothetical reasons, why sal ammoniac *ought not* to attract moisture from the air. I shall confront them with the results of experiments

* If mere heat can separate the combined water of sal ammoniac, then the salt, which, after passing through ignited quartz, has concreted on the verge of ignition, being nearly anhydrous, will, in an equal weight, contain more acid than before transmission. It will in fact bear the same relation to common sal ammoniac, that ignited sulphate of soda does to the crystallised salt. Having transmitted, in the state of vapour, the salt condensed from the dry gases, through ignited quartz, I took 10 gr. of the cake, consolidated just beyond the quartz ; and dissolving them in water, decomposed by nitrate of silver, when I obtained 27 gr. of dry muriate of silver, being the quantity precisely equivalent to 10 gr. of ordinary muriate of ammonia. (See WOLL. Scale of Chem. Equiv.) Hence it is evident, that ignited sal ammoniac has undergone no change in its constitution. There was obtained one-tenth of a grain of liquid in that experiment, in which 20 gr. of salt had been passed through the quartz ; but this, though colourless rock-crystal, betrayed the presence of iron in its composition. For, the liquid stained the paper on which it was withdrawn, of a yellow colour ; and the sublimed salt had, faintly, the same hue. The resulting muriate of silver partook, a little, of the brown tinge, of peroxide of iron. I therefore ascribed the production of liquid to the action of the oxide of iron on the hydrogen of the ammonia.

experiments very carefully conducted; and we shall find, that though this salt is not, perhaps in strict chemical language, deliquescent, yet it is capable of absorbing or attracting from the atmosphere a very notable portion of water. This, however, may be totally expelled, by keeping the salt for a short time in a temperature of from 200° to 300° of Fahrenheit, when it resumes exactly its pristine weight.

Pulverised sal ammoniac was desiccated in a platina capsule, at a heat somewhat below that of its sublimation. It was then found to weigh 49 gr. I placed the capsule on a shelf in my apartment. On re-weighing it, at the end of two days, the salt was found to have become heavier by 3.1 gr.; which amounts to more than six parts in the hundred. Sal ammoniac from gaseous combination exhibits the same phenomenon; and probably, from the extreme comminution of the powder, to a still greater degree. It even becomes pasty. The quantity of this adhering or hygrometric water, will vary no doubt with the weather or climate, as is the case with muriate of soda *.

It may be said, Since our sole object in decomposing sal ammoniac, by metallic laminæ, is to obtain from its acid constituent the water which it is supposed intimately to contain, and to carry into that salt; why not employ directly dry muriatic acid gas, in such researches? My only answer is, that in desiccated sal ammoniac, we conveniently find the acid in a state hygrometrically

* In clear frosty weather, or in a very dry apartment, where muriate of lime would crystallise, the ammoniacal salt acquires weight very slowly. Fifty grains of that from gaseous combination, just heated in a platina capsule, to near its subliming temperature, being put into the scale of a sensible balance, became heavier by half a grain in two minutes; after which, in the above circumstances, no further absorption of moisture was perceptible for an hour. The experiments detailed in the text were made, when the air was considerably loaded with moisture.

hygrometrically dry. Unwilling, however, to shelter myself behind such an apology, I have thrice made the experiment with this acid gas, and have obtained each time a most satisfactory result.

Pure iron turnings were introduced into a green glass tube about 4 feet long; and they were made to occupy about 6 inches of it, near one end. To this was luted, the tube of communication with the mercurial trough. The part containing the turnings, rested on the semicylinder of iron, within the furnace. To the other end, which was about 3 feet from the furnace, the beak of a tubulated retort was luted. Into this, which contained dry muriate of soda, and which was furnished with a syphon-tube of safety, concentrated sulphuric acid was slowly dropped. The projecting green glass tube, with the neck of the retort, altogether about four feet long, were surrounded with paper kept moist with ether, as was also the tube of communication, on the other side, with its bulb. As soon as I found that the gas escaping in the pneumatic trough was muriatic acid, free from admixture with common air, the charcoal furnace was kindled. The heat being gradually raised to bright ignition, whilst the acid gas was slowly disengaged, in the cold retort, I found at the end of a short period, that liquid was condensing in the bulb, though no traces of moisture ever appeared within the long tube, between the retort and the furnace. The moisture increased in a very slow, but regular progression. At the end of three hours the quantity had become considerable. In one case, it amounted to nearly 6 grains. It was liquid muriatic acid. During the whole process, hydrogen and muriatic acid gases escaped through the mercury, in the proportion, by volume, of 13 of the former to 1 of the latter.

This acid gas, as it issues hot from the furnace, is very apt to take up the deposited moisture, unless we screen the tube from

from the fire, and attend diligently to the application of the ether. Dry muriatic acid gas, and common air, do not affect iron in the cold, at least during a period equal to their joint application in the present case, as I have found by experiment in a graduated glass-tube over quicksilver ; and as the whole atmospheric air was expelled, before the application of heat, the effect is solely due to the acid gas, as, indeed, its progressive increase, with the duration of the process, sufficiently attest. The oxygen of the atmosphere could have no influence on the result *.

There was found within the tube, near the end farthest from the retort, on the verge of the ignited part, a white pulverulent matter, glistening like snow, and the adjoining laminæ of iron, were encrusted with the same substance, in spangling crystalline plates.

This powder dissolves readily in water. Into recently boiled water, when a little of it was put, and a small fragment of crystallised prussiate of potash was added, a greyish cloud appeared, speedily becoming blue. Tincture of galls dropped into a similar solution, gave the characteristic purple tinge of iron. Thirteen parts of it by weight, being ignited in a small platina tray, evolved copiously a dark-brown smoke, smelling of muriatic acid, and left 6 parts, which were red oxide of iron. But 13 of green muriate, by Dr WOLLASTON's scale, are equivalent to 8 of red oxide. Does the above muriate contain the atomic

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* M. GAY LUSSAC, in his *Recherches Physico-chimiques*, describes a similar experiment, but without the production of water. If the utmost precautions be not taken, to keep the condensing tube at a very low temperature, the expanded and heated acid gas will readily carry off the moisture, as I found in one experiment. The neglect of these precautions will account for the difference of M. GAY LUSSAC's result.

atomic protoxide of iron, hitherto unknown? This question must be answered by future researches.

I tried to determine the proportion of muriatic acid in the above muriate of iron, by nitrate of silver. The quantity on which I operated was, however, too small to permit me, to place much confidence in the result. But one circumstance arrested my attention. A portion of revived silver was found at the bottom of the glass capsule, on pouring off the precipitated muriate.

On the general principle of research, above stated, the product of water or liquid, will be proportionate to the quantity of muriatic acid gas, condensed into muriate of iron. Hence, to obtain large results, it is proper to have a considerable portion of iron laminæ, placed just at, or a little beyond, the limit of ignition.

From the whole of the preceding experiments, we may legitimately conclude, that muriatic acid gas hygrometrically dry, contains much combined water. And since that gas results from the union of chlorine and hydrogen in equal volumes, each likewise hygrometrically dry, the above water must be formed in consequence of the hydrogen finding oxygen in the chlorine, for its saturation. Chlorine is therefore Oxymuriatic, or Oxygenated muriatic acid.

My experimental examination of iodine, has further led me to conclude, that this curious substance is not entitled to rank in the same class with chlorine, but with sulphur. The details will form the subject of a separate memoir.

XVIII. *On the Laws which regulate the Distribution of the Polarising Force in Plates, Tubes, and Cylinders of Glass, that have received the Polarising Structure.* *By* DAVID BREWSTER, LL. D. F. R. S. LOND. & EDIN.

(Read June 17. 1816.)

IN the Philosophical Transactions for 1816, I have described at great length the various phenomena which are exhibited by glass and other substances to which the property of double refraction has been communicated by heat, by rapid cooling, by evaporation, or by mechanical compression and dilatation. In pursuing the same subject, I have observed many singular facts respecting the developement of new axes, by a change in the form and condition of the plates; and by submitting the phenomena to accurate measurement, I have succeeded in determining the laws which regulate the distribution of the polarising force. A brief account of these results will form the subject of the following paper.

1. *On Plates of Glass with One Axis of Polarisation.*

If we take a plate of glass perfectly circular, and communicate to it the polarising structure, either transiently, by the transmission of heat from its circumference to its centre, or permanently, by cooling it rapidly, when it has been made red hot, we shall find that it will exhibit, when exposed to polarised light, a system of rings traversed by a black rectangular cross. This system of rings is precisely the same, both in appearance and in the character of its tints, as the system seen

along the axis of *Ice*, *Quartz*, &c. and other crystals of the *positive* class.

If the circular plate of glass, on the contrary, receive the polarising structure transiently, by heating it uniformly in boiling oil, and allowing it to cool rapidly, it will exhibit a similar system of rings; but this system has a negative polarisation, like the rings formed by *Calcareous spar*, *Beryl*, &c. and other crystals of the *negative* class.

This opposition in the character of the two plates may be finely observed, by combining them together. The resulting system of rings, when two positive or two negative plates are combined, will be the same as that which would have been produced by a plate equal to the sum of their thicknesses; but when the one is positive, and the other negative, the resulting system will be that which would be produced by a plate equal to the difference of their thicknesses. Hence, when the negative system is exactly equal to the positive system, they will destroy each others effects, and the compound plate will have no action whatever upon polarised light.

By comparing the value of the tints with their distances from the centre of the plate, I have found, that they vary as the squares of their distances from the axis. Hence if T is the tint which corresponds to any distance D, the tint t corresponding to any other distance d, will be found by the formula

$$t = \frac{T\,d^2}{D^2}.$$

2. *On Plates of Glass with Two Axes of Polarisation.*

When a plate of glass deviates from the circular form, and is either elliptical or rectangular, it has two axes of polarisation, one of which is perpendicular to the plane of the plate, and the other at right angles to it, and lying in the plane of the plate. When the plate has received the polarising structure transiently, by the transmission of heat, or permanently, by being

being cooled rapidly from a red-heat, the axis perpendicular to the plane of the plate (which is always the principal axis), is *positive;* but when the polarising structure is communicated by heating the plate in boiling oil, and then cooling it rapidly, the principal axis is *negative.*

By measuring carefully the distances of the tints from the centre of the plate, I have found the following formula, deduced from the supposition of two axes, perfectly correct, viz. $t = T - \frac{T\,d^2}{D^2}$, where D is the distance of either of the black fringes or lines of no polarisation from the centre of the plate. The term $\frac{T\,d^2}{D^2}$ represents the influence of the principal axis, and would have given us the tint t if that axis had existed alone. But as the axis in the plane of the plate produces an uniform tint T in every part of the plate, which acts in opposition to the other tint; the tint t must be equal to the difference of these tints, or to $T - \frac{T\,d^2}{D^2}$.

In examining the relative intensities of the two axes in rectangular plates of considerable length, and in elliptical plates, in which the conjugate axis is very small when compared with the transverse axis, I have found that D, or half the distance between the black fringes, is a function of the breadth of the plate, that is, if B is the breadth of the plate $2\,D : B = 10 : 16.02$, and $D = .312\,B^2$. As the excentricity of the elliptical plate diminishes, the value of D diminishes, or the polarising force of the axis in the plane of the plate diminishes; and when the conjugate and transverse axes are equal, D is equal to 0, or the axis in the plane of the plate is destroyed. In elliptical plates, the black fringes which are seen when the transverse axis is inclined 45° to the plane of primitive polarisation, are convex towards the transverse axis, and their curvature is such, that they

they cut perpendicularly every similar ellipse drawn within the plate. Hence, when the ellipse is nearly a circle, the distance of the fringes will be almost nothing, and each of the fringes will form two straight lines at right angles to one another, as in elliptical plates, when the transverse or the conjugate axis is in the plane of primitive polarisation.

The lines of equal tint at the angles of rectangular plates of glass, are highly deserving of attention. They are produced by the external fringes, and the maximum tint at the angles is always less than at the edges, and generally higher than the maximum tint in the middle of the plate. When the external fringes are two in number, viz. $a\,b$, $a'\,b'$, $d\,e$, $d'\,e'$, as shown in Plate VII. Fig. 1.; the outermost, $a\,b$, $a'\,b'$, terminates at c, c, and the other, $d\,e$, $d'\,e'$, branches off at e, e', so as to form the ellipsis $e\,f\,e'\,e$. The tint increases from this elliptical line to its centre o, but decreases from o to p, and from o to q. When the glass plate is a perfect square, the curve $e\,f\,e'\,e$ is a circle *. In a plate whose maximum tint, in the middle, was a *Blue of the 2d order*; the tint at o was a *Pink of the 2d order*; and that at p and q a *White of the 1st order*.

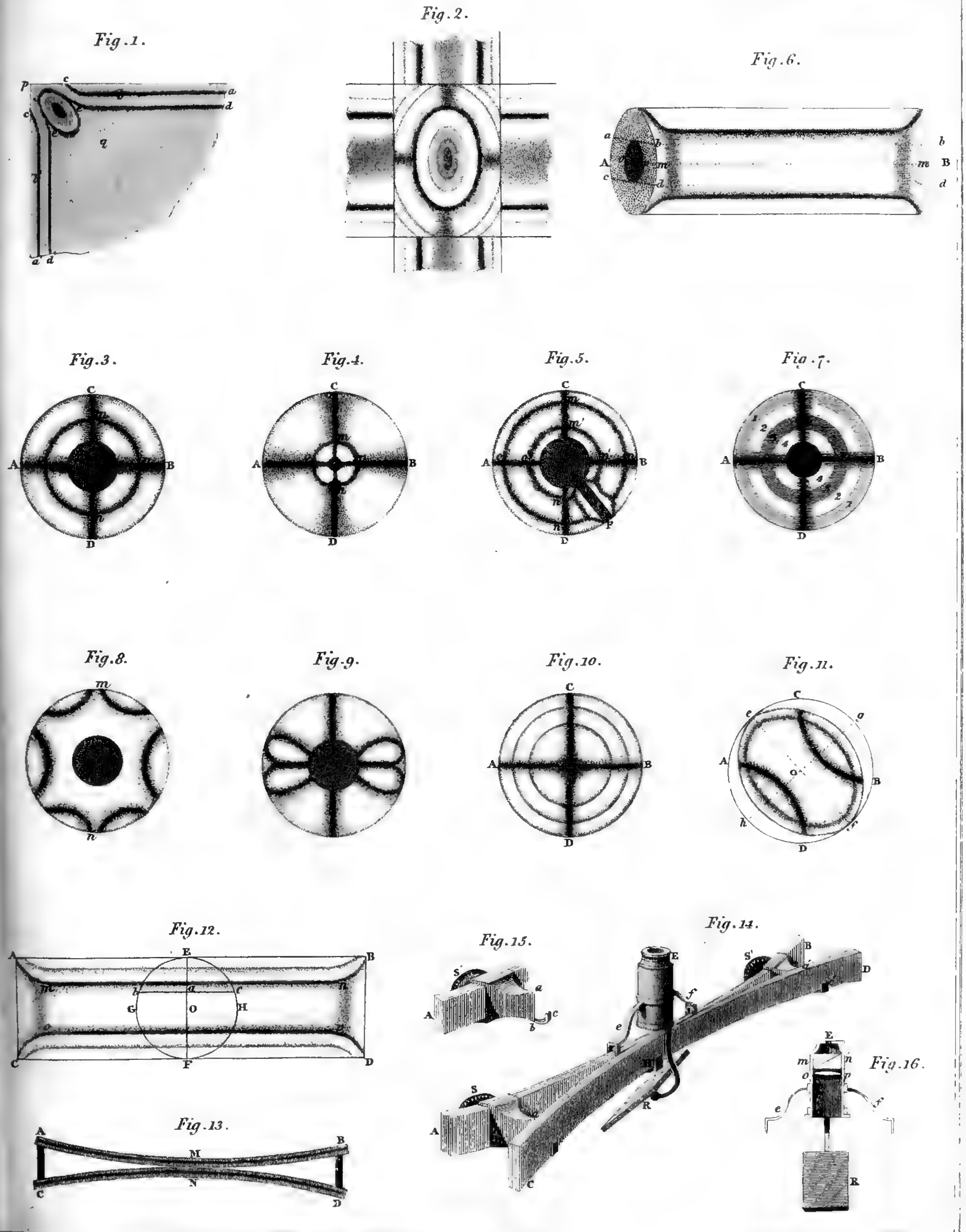
3. *On the Lines of Equal Tint formed by the Transverse Combination of Plates of Glass.*

In the Philosophical Transactions for 1816, I have represented the *Isochromatic lines*, or lines of equal tint, formed by the transverse combination of plates of glass, when the principal axes are both negative, or both positive †; when the principal axis in one is negative, and in the other positive ‡; when the two plates receive their structure by bending ‖; and when a bent plate is combined with a plate formed by heat §. In these various cases, the lines of equal tint

* See *Phil. Trans.* 1816, Plate IV. fig. 29. † *Id.* Plate II. fig. 3, 4. ‡ *Id.* Plate II. fig. 8. ‖ *Id.* Plate IX. fig. 9. § *Id.* Plate IX. fig. 10.

PLATE VII.

Engraved for the Royal Society Tran. Vol. VIII. Page 356



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tint will be found to be *Hyperbolas*, *Circles*, *Ellipses*, *Straight Lines* and *Parabolas*.

Let us now suppose,

$T\ T' =$ the maximum tints of the two plates.
$B\ B' =$ the breadths of the plates.
$x =$ the distance from the centre of the plate of any point where the resulting tint is required.
$y =$ the distance of the same point from the centre of the other plate.
$t\ t' =$ the tint produced by each plate separately at the distances x and y, and
$\tau =$ the resulting tint.

Then, substituting .312 B instead of its equal D, we have

$$t = T - \frac{T\,x^2}{.312\,B^2}, \text{ and } t' = T' - \frac{T'\,y^2}{.312\,B'^2}.$$

But, since the resulting tint τ arising from the combination is equal to the difference of the two tints, we have

$$\tau = T' - T - \frac{T'\,y^2}{.312\,B'^2} + \frac{T\,x^2}{.312\,B^2}, \text{ and}$$

$$y^2 = .312\,B'^2\left(\frac{T' - T - \tau}{T'}\right) + \frac{T.312\,B'^2\,x^2}{T'.312\,B^2}.$$

Consequently, the lines of equal tint are *Hyperbolas*. When $T = T'$, and $B = B'$, the hyperbolas are equilateral, and

$$y^2 = .312\,B'^2\left(\frac{-\tau}{T}\right) + x^2.$$

When a plate whose principal axis is negative, is crossed with a plate whose principal axis is positive *, the resulting tint

* *Phil. Trans.* 1816, vol. II. fig. 8.

tint will be the sum of the separate tints. Hence, in this case we have

$$\tau = \mathrm{T} + \mathrm{T}' - \frac{\mathrm{T}\, x^2}{.312\, \mathrm{B}^2} - \frac{\mathrm{T}'\, y^2}{.312\, \mathrm{B}'^2}, \text{ and}$$

$$y^2 = .312\, \mathrm{B}'^2 \left(\frac{\mathrm{T}' + \mathrm{T} - \tau}{\mathrm{T}'}\right) - \frac{\mathrm{T}\, .312\, \mathrm{B}'^2 x^2}{\mathrm{T}'\, .312\, \mathrm{B}^2}.$$

The lines of equal tint will therefore be *Ellipses*, as shewn in Plate VII. Fig. 2. when either the maximum tints, or the breadths of the combined plates are not equal.

But, when $\mathrm{T} = \mathrm{T}'$ and $\mathrm{B} = \mathrm{B}'$, we have

$$y^2 = .312\, \mathrm{B}'^2 \left(\frac{2\,\mathrm{T} - \tau}{\mathrm{T}}\right) - x^2.$$

Consequently, the lines are in this case *Circles*, as shewn in *Phil. Trans.* 1816, Plate II. Fig. 8.

When the two combined plates receive their structure by bending, the tints do not vary, as the squares of their distances from the centre, but simply as their distances. Hence, T being the maximum tint at the edge of the plate, and B B′ the breadths of the plate as formerly, we have $\frac{\mathrm{B}}{2} : \mathrm{T} = x : t$; and

$\frac{\mathrm{B}'}{2} : \mathrm{T}' = y : t'$, which give

$t = \frac{2\,\mathrm{T}\, x}{\mathrm{B}}$, and $t' = \frac{2\,\mathrm{T}'\, y}{\mathrm{B}'}$. Consequently,

$$\tau = \frac{2\,\mathrm{T}\, x}{\mathrm{B}} + \frac{2\,\mathrm{T}'\, y}{\mathrm{B}'}, \text{ and}$$

$$y = \frac{\mathrm{B}'\, \tau}{2\,\mathrm{T}'} - \frac{\mathrm{T}\,\mathrm{B}'\, x}{\mathrm{T}'\,\mathrm{B}}.$$

Hence, the lines of equal tint will be *Straight lines.*

The

The angle φ, which the straight lines of equal tint form with the edges of the plate, will be found by the formula,

$$\text{Tang. } \varphi = \frac{T}{T'} = \frac{B}{B'}.$$

When $x : y = B : B'$, and when similar sides of the plates cross each other, we shall have $\tau = 0$, that is, the line of no-polarisation will be the diagonal of the parallelogram formed by the sides of the two plates *.

When $B = B'$, and $T = T$, then

$$y = \frac{B\tau}{2T} - x, \text{ and}$$

the straight lines of equal tint will be inclined 45° to the edges of the plates, for $\frac{T}{T'} = 1$, which is the tangent of 45°.

When a plate of glass with two axes is combined with a plate of bent glass †, we have

$$\tau = \frac{2T'y}{B'} + T - \frac{Tx^2}{.312\,B^2}, \text{ and}$$

$$x^2 = .312\,B^2 \left(1 - \frac{\tau}{T} + \frac{2T'y}{TB'}\right),$$

when the *concave* side of the bent plate crosses a plate with two axes, in which the principal axis is *negative*; or,

$$\tau = \frac{2T'y}{B'} - T + \frac{Tx^2}{.312\,B^2} \text{ and}$$

$$x^2 = .312\,B^2 \left(1 + \frac{\tau}{T} - \frac{2T'y}{TB}\right),$$

when the *convex* side of the bent plate crosses a plate with two axes, in which the principal axis is *positive*, and *vice versa*. Hence, it follows, that the lines of equal tint are here *Parabolas*.

 When

* See *Phil. Trans.* 1816, Plate IX. fig. 9. † *Id.* Plate IX. Fig. 10.

When T = T′ the line of no-polarisation is a complete parabola, passing through the angles of the parallelogram, and having its vertex at the very edge of the bent plate. The curves within it will also be complete parabolas, but all those without it will be only parabolic segments.

4. *On the Distribution of the Polarising Force in Tubes and Cylinders of Glass.*

We have already seen, that circular plates, or cylinders of glass, have *one negative axis*, like *Quartz*; but when the cylinder has the form of a tube, like AB, Plate VII. Fig. 3. the polarising force is distributed in a very remarkable manner. A black circular fringe *m p n o* forms the line of no-polarisation, and the coloured fringes are placed on each side of this dark ring, and concentric with it. The structure on the outside of *m p n o* is *negative* like *Calcareous spar*; and the structure on the inside *positive*, like *Quartz*, &c.; and the effect is exactly the same as if a plate of glass had been bent into a circular form, and kept in that position by force.

The breadth of the positive annulus *a o* is always less than that of the negative annulus A *o*, the former decreasing, and the latter increasing, as the bore of the tube diminishes; and when the bore becomes extremely small, as in Fig. 4. the positive structure is also extremely small, and sometimes can scarcely be seen without the aid of a microscope.

In comparing the values of the tints with their distances from the line *m p n o*, I have found that they vary as the distances, in the same manner as in bent glass. Hence it is obvious, that the glass is in a state of compression within the black ring *m p n o*, and in a state of dilatation without that ring, and that the particles are held in a state of violent constraint, entirely different from that position of equilibrium in which they are placed in plates of glass with one or two axes.

With

With the view of confirming these results, I took a file with a very sharp edge, and cut the tube entirely through by a notch EF, Fig. 5. By this operation the particles were freed from the state of violence in which they were held, and assumed the very same arrangement which they never fail to take in rectangular plates of glass. By exposing the tube, thus divided, to polarised light, it exhibited the appearance shewn in Fig. 5. where $m\,p\,n\,o$, $m'\,p'\,n'\,o'$, are two dark fringes having a negative structure on the outer side of each, and a positive structure between them, as in plates of glass with two axes. I obtained the same result with a tube of a very large bore, having its exterior diameter 0.875 of an inch, and its interior diameter 0.816; so that the thickness of the glass was only 0.059 of an inch. When the tube is cut into two or more pieces, each piece has the same structure as a plate of glass with two axes. By a number of measurements, I have found that the diameter $o\,p$, Fig. 3. of the black fringe or circle of no-polarisation, is a geometrical mean between the interior and exterior diameters of the tube, that is, $o\,p = \sqrt{\mathrm{AB} \times a\,b}$.

If a tube of glass is brought to a red heat, and then cooled by inserting in its bore a cylinder of iron, or any other conducting body, the structure will then be the same as is represented in Fig. 5.

If a solid cylinder of glass which has only one structure, is perforated in its centre, it will exhibit the appearance in Fig. 3., and if it is divided by a notch, it will acquire the structure shewn in Fig. 5.

When polarised light is transmitted through a long cylinder of unannealed glass, in a line perpendicular to its axis, after it is immersed in a fluid of the same refractive power, it exhibits the same phenomena as rectangular plates of glass, having a positive axis perpendicular to its length, and a negative axis perpendicular to the positive one; but in the cylinder, the ex-

ternal tints do not rise so high, from the diminution in the thickness of the glass.

The same phenomena are exhibited by a glass-tube AB, Fig. 6. similarly placed. In this case, however, the maximum tint does not appear along the line *m m*, the axis of the tube; but it is seen both in the lines *b b* and *d d*, equidistant from *m m*. This effect is obviously occasioned by the greater thickness of the glass in these directions; for *a b* and *c d* are each greater than A $o + p\ m$, and the difference is sufficiently great to overbalance the diminution of the tints at a greater distance from the principal axis.

In examining very carefully the structure of glass tubes, when exposed to polarised light, it will be found that they are as it were divided into different elementary concentric tubes; and that in some cases there is an actual separation between them. Hence, there arises a remarkable singularity in the progression of the tints. Instead of shading into one another by imperceptible gradations, each elementary tube has an uniform tint of its own, as is represented in Fig. 7. where the tube AB is divided into four tubes 1, 1; 2, 2; 3, 3; 4, 4; the tube 4, 4 is in every part of it a white of the first order; the tube 3, 3 is every where equally dark, being the black circular fringe *m p n o* of Fig. 3.; the tubes 2, 2 and 4, 4 are a white of the first order; and the tube 1, 1 is a yellow of the first order. Hence it follows, that in tubes which possess this peculiarity of structure, each elementary tube is uniformly dense throughout, and that the variation of density takes place by leaps.

If a portion of a glass tube perfectly annealed, is exposed to pressure, it exhibits the tints shown in Fig. 8. when the line *m n*, joining the points of pressure, is parallel or perpendicular to the plane of primitive polarisation; and the tints shown in Fig. 9. when the same line is inclined 45° to that plane.

5. *On*

5. *On the Conversion of Plates of Glass with One Axis into Plates with Two Axes.*

From the different experiments which I have described, both in this and in other papers, it appears, that in almost every case where polarising forces are developed, two negative structures are separated by a positive structure, or two positive structures by a negative structure, both of which are simultaneously produced, like the two opposite polarities in electricity and magnetism. When the two structures are disposed in a different manner, or when only one structure is developed, the regular arrangement of the polarising forces may be effected by a slight change in the form or in the mechanical condition of the plate.

If we take a piece of unannealed glass perfectly circular, we have only one axis, or one structure, as shewn in Fig. 10., where AB, CD is the black cross, which preserves the same appearance by turning the plate round its centre. But if we grind the smallest quantity from any two opposite sides CB, AD, so as to induce a slight degree of ellipticity, a new axis is created; the tint which it produces appears at the centre of the plate, and the system of rings has the form shewn in Fig. 8., where the internal structure within the black fringes AD, CB, is negative, and the two external structures without AD, CB, positive.

If, on the contrary, we now grind a small quantity from the sides AC, BD, so as to reconvert the elliptical plate into a circular plate, we shall find that the new axis which was generated by the change of form, has entirely disappeared, so that the plate exhibits the figure shewn in Fig. 10.

This singular experiment, in which one of the axes may be extinguished and revived at pleasure, is worthy of the most attentive

attentive consideration. If the polarising forces depend solely on the mechanical condition of the particles of the glass, then it necessarily follows, that the central parts of the glass, which were in a state of variable expansion when it had a circular shape, are in a state of variable compression when the glass has received an elliptical form, and we are presented with a new law relative to the equilibrium of the cohesive forces in solids of variable density. But if the variation of density is merely the means of developing a new agent in the same manner as heat excites electricity in the tourmaline, or as pressure excites it in calcareous spar, then we cannot avoid regarding this agent in the same light as the electrical and magnetical fluid which are decomposed by certain mechanical operations, and distribute themselves according to regular laws.

But whatever be the origin of the polarising forces, it becomes a matter of great importance to discover the law of their distribution, when they are not controuled by opposite actions, and to apply this law to the explanation of the phenomena which they develope when they are either modified or extinguished by the external form of the body in which they reside.

Those who have studied the papers to which I have already referred, cannot have failed to remark, that when the polarising forces are unconstrained in their developement, a negative structure is generated in the middle of the plate, when a positive structure is generated at one or both of its edges; and that the intensity of the negative, is to the intensity of either of the positive structures, as 10 to 16.02.

In order to apply this principle to a circular plate, let EHFG, Fig. 12. be a plate of this description, whose centre is O. Then if this plate were a part of the rectangular plate ABDC, *mn*, *op* would be the lines of no-polarisation which separate the internal negative

negative structure from the two external positive structures. The tint at a, or any part of the line $b\,c$, would be $T - \frac{T\,d^2}{.312\,B^2}$; but if the circular plate were part of a plate similar to, and at right angles to ABCD, the tint at a, or any part of the line EOF, would be equal to T; and as this tint is rectangular to the other tint at a, the resulting tint will be equal to the difference of these tints, or to

$$T - T - \frac{T\,d^2}{.312\,B^2} = \frac{T\,d^2}{.312\,B^2}.$$

In like manner, it may be shewn, that in every point of the circular plate, the tint is represented by $\frac{T\,d^2}{.312\,B^2}$, which is the experimental expression for it already found. In plates, therefore, that have only a positive structure, the negative structure still exists, but is overpowered by opposite actions.

We are now prepared to understand how the negative structure re-appears, as shewn in Fig. 11., by giving an elliptical form to the plate. For, the maximum negative tint produced at O, in the direction $g\,h$, is no longer counterbalanced by the tint in the direction $e\,f$; and therefore the difference of these tints appears at O, with a negative character. As the points e, f remove from O, or as the ellipticity increases, the tint at O gradually rises till it becomes equal to T, or $\frac{10}{16.02}$ times the tint at g, when the action of the edges at e and f has no longer any influence at O. The very same results are obtained by the conversion of a sphere into a spheroid, and they are explicable upon the same principles.

The

The distribution of the polarising force at the angles of square or rectangular plates, characterises a very remarkable state of equilibrium among the cohesive forces; and this state of equilibrium is not an accidental result of the mode in which the heat either enters or leaves the angles of the glass plate, for the very same distribution takes place when a new angle is formed, by cutting the plate into two parts *. In all these cases, the lines of equal tint are the lines of equal density.

As the two kinds of polarising forces seem to be co-existent in every part of a glass plate, and as each of them, when it appears to exist alone, is merely the resultant of two opposite forces, it is easy to assign a reason for the singular phenomena which are exhibited, by dividing a plate of glass into two parts. The two forces which reside in every part of the glass cannot be in equilibrio, unless a negative structure is placed between two positive structures; and therefore each half of the glass plate, or each portion of it of the same shape as the whole plate, must acquire the same property as the plate itself, or have the forces distributed in the same manner. This view of the distribution of the polarising forces is analogous to COULOMB's theory of the construction of the magnet. Every elementary portion of the magnet has a north and south pole, and therefore wherever it is broken, the fragment must have a north and south pole, like the magnet of which it formed a part.

6. *On*

* See *Phil. Trans.* 1816, p. 82.

6. *On the Intensity and Distribution of the Polarising Force in Plates of Bent Glass.*

In order to ascertain the intensity of the polarising force, as produced by different degrees of flexure, I placed together two similar plates A B, C D, Fig. 13, and separated their extremities by two pieces of glass A C, B D, of equal thickness. After pressing them into contact at M and N, I observed the maximum tints which they exhibited at these points, as given in the following Table.

Length of Plates, or A B.	Distance of Extremities, or A C.	Thickness of the Glass.	Breadth of the Plate.	Maximum Tint.
16 inches	0.16	0.133	0.35	4
13	0.16	0.133	0.35	9
12	0.048	0.207	0.967	10.6
6	0.054	0.133	0.35	10.5

With the view of ascertaining the tints which they yielded before they broke, I took plates, whose thickness was 0.1333 of an inch, and breadth 0.33. One of these broke when the tint was 10.4, another when the tint was 12, and a third when it had reached 13.55. In the phenomena of bent glass, the polarising force is distributed in such a manner, that the lines of equal tint are the lines of equal compression or dilatation, and the tint at the edge is every where inversely proportional to the radius of curvature.

In order to compare the effects produced by the application of the same force to plates of different thicknesses, I placed them as in Fig. 13. The thickness of A B was .133, and its tint 3.4.

 The

The thickness of C D was .230, and its tint 10.2. But $133^2 : 230^2 = 3.4 : 10.2$; so that the tints and the elasticities were, in this case, inversely as the squares of the thicknesses.

The preceding experiment furnishes us with the principle of a new instrument, which may be called a *Teinometer*, for ascertaining the elasticities of bodies. The tints of AB and CD are obviously measures of the elasticities of the two plates; so that the elasticities of plates of glass of different dimensions, or formed of different materials, may be readily determined. The power of the instrument however, is not limited to glass; for, by using a glass plate AB as a standard, the other plate CD may be a plate of metal, or any other opaque substance, whose elasticity it is required to ascertain. The tint of AB, when opposed by a plate of equal elasticity, is known by experiment; and therefore, the tint which it affords, when opposed by a similar plate, of a substance possessing a greater or a less elasticity than itself, is a measure of this elasticity. Although I consider the variation of the tint as an excellent means of determining the degree of curvature, yet the principle o exhibiting the relative elasticities of two plates, by the application of the same force, may be employed in an instrument entirely mechanical, in which the *sagitta* of the inflected plate is actually measured, as in the ingenious machine invented by S'GRAVESANDE.

The chromatic *Teinometer* is represented in Figs. 14, 15. and 16. where AB is the standard plate, of well annealed glass, having its edges highly polished. Along this plate, there are moved two brass pieces, S *a b c*, S′ *a′ b′ c′*, which can be fixed in any position, by means of the screws S, S′. The plate CD, where elasticity is to be measured, rests with its lower edge upon the projection *b c*, Fig. 15. and with one of its faces against *a b*. It is then pressed into contact with the plate AB, and

and kept in this position by the wooden holdfast H; the brass pieces S *a b c*, S′ *a′ b′ c′*, having been previously placed at such a distance from each other, that the two plates will meet at H, without breaking, or without any permanent change of form. The apparatus ER, for observing the tint, is shewn separately in Fig. 16. It consists of an eye-piece E, to which is attached a reflector R, made of several plates of the thinnest glass, about 1½ long and an inch broad, and placed close to each other. The eye-piece E consists of two tubes, one of which is moveable within the other. The moveable tube contains an achromatic prism, *m n*, of calcareous-spar, with a convex lens, *o p*, about an inch in focal length, placed either above or below it. When this apparatus is set upon the edge of AB, by means of the forked arms *e*, *f*, the reflector R is turned round, till the plane of reflection is cut at an angle of 45°, by the plane of the plate AB, and is placed at such an angle, that the light which it reflects through the edge of the plate AB, and up the tube, is completely polarised. The moveable tube is then turned round, till the tints appear on the edge of one of the images of the glass plate. In order to avoid the confusion arising from two images, the achromatic prism may be constructed in such a manner that only one of the images is visible.

XIX. *Remarks, illustrative of the Scope and Influence of the Philosophical Writings of Lord Bacon.* *By* MACVEY NAPIER, Esq, F. R. S. Lond. & Edin. and F. A. S. Edin.

(Read February 16. 1818.*)*

THE obligations of Experimental Physics to the labours of Lord BACON, have been largely acknowledged by the generality of those who have treated of the History of Modern Science; insomuch, that the title of *Father of Experimental Philosophy* has been oftener conferred upon him than upon any other of its benefactors. There are some, however, who seem to think, that there is no good ground for honouring him with this title, either on account of the merits or the effects of his writings. They do not indeed deny, that his views as to the proper objects and method of philosophizing were extensive and just; but they contend, that he had no *peculiar* merit in having stated these views; that all that he taught was virtually and more effectually taught by the discoveries of some of his contemporaries; and that, in fact, there are no traces of his agency to be found in the discoveries that followed *. These opinions, though they are to be met with in respectable

* " Atqui VERULAMIUS ille, qui Germanæ Philosophiæ Restitutor, quin etiam, si Superis placet, Parens a BRUKERO aliisque habetur, quid aliud in Anglia præstitit,

able books, and in the conversation of intelligent men, seem to involve no small portion both of error and misconception. It cannot be denied, indeed, that at the time when Bacon wrote, there was a growing tendency to abandon the ancient systems, and that some successful essays had been made in that course of inquiry which he recommended; but, on the other hand, it appears to me equally clear, that his labours for the advancement of Science were of such peculiar importance, and attended with such extensive effects, as to entitle him to a pre-eminent station among its early reformers and promoters. It is the object of this paper to offer some remarks, and to collect some proofs, in support of these views; but, as much has been already written in illustration of the merits, and but little in illustration of the *effects* produced by his philosophical writings, I shall content myself, at present, with a slight indication of their general scope, and shall devote the greater part of my paper to the proofs of their influence. Upon the latter point, indeed, there seems to exist more of doubt and of misapprehension than upon any other connected with his philosophy*.

In

tit, nisi, ut, qua ratione philosophari deberemus, eo tempore admoneret, quo Galilæus eadem ipsa ratione philosophari jam in Italia cœperat, ac cæteris, ut idem facerent, non modo verbis, verum et rebus ipsis gravissimus auctor esset?"—Fabroni, *Vitæ Italorum doctrina excellentium qui sæculis* xvii. *et* xviii. *floruerunt*, vol. i. p. 223.

——"C'est Galilee," says a French Philosopher of the present day, "qui a montré l'art de l'interroger par l'expérience. On a souvent attribué cette gloire à Bacon; mais ceux qui lui en font honneur, ont été (à notre avis) un peu prodigues d'un bien qu'il ne leur appartenait peut-être pas de dispenser."—*Biographie Universelle*, Tom. xvi. p. 329, Art. Galileo; written by M. Biot.

* There cannot be a stronger proof of the misapprehensions alluded to, than what is furnished in the following passage, of the interesting article above mentioned.

In order to clear the way for this inquiry, I shall begin with a few remarks on a late estimate of BACON's Philosophy, evidently intended, not merely to depreciate, but to vilify it; insomuch, that it stands remarkably at variance with almost all that has been hitherto published on that subject. The estimate alluded to is the more worthy of notice, that it has obtained a place in a Literary Journal of great respectability, and which is supposed by many to speak the sentiments of the English Universities in matters of philosophy.

It is pretty well known, I presume, that BACON's writings have been recently commented upon by two of our most eminent philosophers; by the one, in reference to their connection with the Philosophy of the Mind *; and by the other, in reference to their more apparent object, the explaining the method by which to extend our knowledge of the Material World †. Both of them represent BACON as the first who clearly and fully pointed out the legitimate rules and ends of philosophical inquiry;

mentioned. "Si BACON a eu tant de part aux découvertes qui se sont faites après lui dans les sciences, qu'on nous montre donc un seul fait, un seul résultat de son invention, qui soit de quelque utilité aujourd'hui: ou, si ses principes généraux sont tellement feconds, qu'ils aient pu, comme on l'assure, lui faire pressentir un grand nombre de decouvertes modernes, il est présumable qu'on n' a pas encore épuisé tout ce que contient son Livre; et dans ce cas, ceux qui disent que nous lui devons tant de choses, devraient essayer d'en tirer d'avance quelques-unes des découvertes dont la methode de GALILEE nous enrichit tous les jours."—*Biog. Universelle*, in loc. cit.

* See Mr STEWART's *Dissertation on the Progress of Metaphysical, Ethical, and Political Philosophy*, prefixed to the *Supplement to the Encyclopædia Britannica.*

† See Professor PLAYFAIR's *Dissertation on the Progress of Mathematical and Physical Science*, prefixed to the same work.

quiry; and both consider his writings as fixing a new and important era in the history of Modern Science. The observations made by the former upon these points, have been examined at considerable length in an able article of the Journal referred to; and the following passage contains the sum of what is there advanced in regard to the general scope and character of BACON'S Philosophy. "The topic on which Mr "STEWART chiefly dwells, while panegyrizing the Philosophy "of BACON, is the respect which it pays to the *limits, the laws,* "*and resources of the human understanding;* and this is sure-"ly the most extraordinary topic of any which he has select-"ed. There is *scarcely a page* in the *Novum Organum*, that "does not furnish a contradiction to it.—So little, indeed, "can BACON be considered as having risen in any great de-"gree above the age in which he lived, with respect to his "views as to the proper aim of philosophy, or the proper "limits of the human understanding, that he even goes "so far as to give us formal receipts for the making of "gold, and performing other prodigies, which he tells us he "judges very possible. With the exception of the disciples of "RAYMOND LULLY and JORDANO BRUNO, *the extravagant spe-*"*culations in which* BACON *wished to embark philosophy, had* "*been long abandoned by sober inquirers* *."

It

* *Quarterly Review*, No. xxxiii. p. 50.—The writer of this article seems to have been anxious to find some great names to countenance what he has advanced in regard to the very inferior merits of BACON'S philosophical writings. What his success has been in this endeavour, the following extract will shew.

"I remember, said Sir JOSHUA REYNOLDS, that Mr BURKE, speaking of BACON'S *Essays*, said, he thought them the best of his works. Dr JOHNSON was of opinion, that their excellence and their value consisted in their being observations of a strong mind operating upon life; and in consequence you find there

It is to be wished, that this writer had explained to us, to what delusion it has been owing, that so many enlightened persons have, for more than a century and a half, concurred in extolling BACON, for his endeavours to withdraw philosophy from "extravagant speculation," and to give it a direction and a method, calculated to improve the condition, as well as the knowledge, of mankind. Have they *all* been in error, and must BACON be branded with the imputation of ignorance of the business of philosophy, and the limits of the understanding, merely because he has speculated upon the possibility of making gold? Is this circumstance enough to establish an affinity between the general aims of his philosophy and the extravagant pursuits of the Alchymists? A very few words will suffice upon this point.

 There

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there what you seldom find in other works."—*Account of Sir JOSHUA REYNOLDS*, prefixed to *MALONE's* edition of his *Discourses.*

"We are glad," the Reviewer adds, "to be able to defend our opinions concerning the inferior merits of BACON's philosophical writings, compared with his other works, from the charge of singularity or presumption, by sheltering ourselves under the authority of such names as BURKE and JOHNSON."

It is very observable, that, so far as Dr JOHNSON's authority is concerned, he does not appear, in the conversation referred to, to have made any *comparison* whatever between BACON's *Essays* and his other works: he only made a remark descriptive of the *Essays*, in which every one who has perused them will readily concur; and besides, the Reviewer ought to have known, that JOHNSON has, in one of his papers in the *Adventurer*, represented BACON as the only Modern worthy of being compared, in a philosophical point of view, with NEWTON; thereby showing, that he must have held the philosophical works of the former in the highest possible degree of estimation. Great as the excellence of the *Essays* undoubtedly is, it is difficult to believe, that such a man as BURKE could deliberately rate them as of higher merit than the *De Augmentis Scientiarum* and *Novum Organum.* There is need of some better evidence, surely, that he had formed a deliberate opinion to that effect, than what is furnished by the scrap of conversation which forms the Reviewer's only document of proof.

There can be no doubt, that BACON did believe in the possibility of discovering the means of converting other substances into gold; a belief, which was far from being so completely abandoned by all "sober inquirers," as this writer imagines; for it was entertained by BOYLE, and some other experimentalists, and not greatly discouraged even by NEWTON, at a period when experimental philosophy was much farther advanced *. There was no man of his day more thoroughly apprised than BACON was, of the follies of the Alchymists, or who has mentioned them in terms of stronger ridicule and reprobation †. He nowhere holds out the making of gold as a prime object of philosophical inquiry; on the contrary, he pointedly censures the Alchymists, with whom he has been so absurdly classed, for directing their main views to such an object ‡. The belief which he entertained as to the possibility of making gold, had a very different foundation from that upon which it rested among this fantastical fraternity. With him, the belief in question formed part of his general belief, that the *essences* of all material substances were capable of being discovered by the inductive process. It was a belief which flowed from his lofty notions of the yet untried resources of experimental science. There was then no sufficient stock of experience to authorise any one to lay it down as an established principle, that the knowledge of these essences is placed

* There is a curious letter upon this subject from NEWTON to Mr OLDENBURG, Secretary of the Royal Society, printed in the account of BOYLE, in the *Historical Dictionary*. His remarks apply wholly to a particular process of transmutation, and not to the *impossibility* of the thing itself. See *General Historical and Critical Dictionary*, vol. iii. p. 558.

† See *Novum Organum*, Lib. i. Aph. 85. 87.

‡ Ibid. Lib. i. Aph. 70.

placed beyond the reach of scientific discovery. It is not very surprising, therefore, that BACON should believe, that a series of skilfully conducted experiments might ultimately lead to the detection of the nature or essence of gold; and that having thus discovered its constituent nature, it would then be possible to superinduce it upon any other substance. There is nothing in all this that any way impeaches his respect to the "laws and limits of the human understanding." He recommended no inquiry upon any other principles than those of *Induction;* and he proposed no object to philosophy, which any thing but experience could shew to be unattainable.

But we are farther told, that there is "scarcely a page in "the *Novum Organum*" which does not afford proofs of BACON's ignorance of the laws and limits of the understanding; and that his miscellaneous philosophical pieces seem to have been written in express contempt of them *. Had this writer contented himself with stating, that there are many things in BACON's miscellaneous pieces, which show that he was not exempt from credulity,—that his understanding, resplendent as it was, bore some stains of the scurf and scum of an ignorant age; or had he only stated that BACON's metaphysical notions are sometimes vague and unsound, and his use of language fanciful and uncertain his observations might have been allowed to pass unnoticed, as neither new nor objectionable. But when he goes so far as to charge the *Novum Organum* as every where manifesting a total ignorance of the fundamental conditions of philosophical reasoning, the only respectful conclusion, I must say, that can be adopted in regard to such an assertion

* *Quarterly Review,* No. xxxiii. p. 50.

tion is, that it has proceeded from a very imperfect acquaintance with the work in question. For my own part, I confess myself wholly unable to conceive, how any man of ordinary judgment could read the *Novum Organum* with ordinary attention, without carrying away an impression directly the reverse of that of BACON's ignorance and disregard of the laws and limits of the human understanding. The first sentence of the work contains an emphatic declaration of homage to these very laws: *Homo Naturæ minister et interpres, tantum facit et intelligit, quantum, de Naturæ ordine, re vel mente observaverit; nec amplius scit, aut potest.* The grand lesson which it every where inculcates is, that all false philosophy had sprung from the too high notions hitherto entertained, of the powers of the mind; these notions having led to the disregard or contempt of the only means by which true knowledge can be obtained. *Causa vero, et radix, fere omnium malorum in scientia ea una est, quod dum mentis humanæ vires falso miramur et extollimus, vera ejus auxilia non quæramus.* BACON saw more clearly than any preceding inquirer, the folly of supposing the mind capable of explaining the constitution of Nature by means of principles of its own invention, and reasonings *a priori;* and his main aim in the *Novum Organum* was, to withdraw philosophy from such airy speculations, and to employ it in a way more suitable to its purposes, and the limited nature of our faculties. Employed in this way, that, namely, of inductive inquiry, he showed that philosophy would greatly extend the compass of our knowledge, and multiply the instruments of our power. It is not, therefore, without good reason, that Mr STEWART panegyrizes the author of the *Novum Organum,* for his knowledge of "the laws, limits and resources of the human understanding," and for the general soundness of his views as to the ends and rules of philosophical investigation.

The

The truth is, that this writer is, after all, constrained to make an admission, which of itself sufficiently proves the groundlessness of his general censure of BACON's philosophy. "That the rules of investigation which it lays down, "*are wise and salutary with reference to physics*, we are "happy," says he, "to admit *." Now, the *Novum Organum* is almost wholly occupied with the exposition and illustration of these very rules; and yet it is branded by this writer with the imputation of manifesting disrespect "in "every page" to the laws and limits of the understanding, and a total ignorance of the purposes of science. It would prove a rather perplexing task, I should imagine, to show how any one could methodize a set of "wise and sa-"lutary rules of investigation with reference to physics," who, yet, had no sound views of the nature and objects of philosophical inquiry. There must either, in short, be something in the nature of *physics* to take that great branch of knowledge out of the general category of philosophy, or it must be absurd to say, that BACON could unfold the true principles of physical investigation, he being at the same time ignorant of the nature and aim of genuine science. His rules with respect to physical inquiry were "wise and salutary," precisely because they were conformable to the laws and limits of the human understanding; because "he saw well," to use his own words, "that the supposition of the too great suffi-"ciency of man's mind had lost the means thereof †."

It is besides to be observed, that there is no ground whatever for the limitation of the wisdom and utility of BACON's logical precepts to the physical sciences alone. He who admits that they

* *Quarterly Review*, No. xxxiii. p. 52.

† *Filum Labyrinthi*, Works, vol. i. p. 400. 4to edit.

they are "wise and salutary with reference to physics," must go a step farther, and admit that they are also wise and salutary with reference to inquiries regarding the mind. The object of philosophy, and the principles of philosophizing are the same, whether the investigation relates to the laws of matter or the laws of mind; and thus the logic of the *Novum Organum* cannot be useful with reference to the one, without having the same character with reference to the other. It is upon this ground that Bacon himself represents his logic as equally applicable to the advancement of the moral and metaphysical as of the physical sciences. "Atque quemadmodum vulgaris Lo-" gica, quæ regit res per *Syllogismum*, non tantum ad naturales, " sed ad *omnes* scientias pertinet; ita et nostra, quæ procedit " per *Inductionem*, omnia complectitur *."

In adverting to the question as to the *influence* of Bacon's philosophical writings upon the subsequent progress of physical science, this writer observes, that it presents a "point " as to which it is very difficult to form an explicit opi-" nion. But this," says he, "is sufficiently clear, that if Ba-" con is to be allowed any considerable share in the honours " which modern experimentalists have acquired, he may, in " many respects, be compared to the husbandman in Æsop's " fable; who, when he died, told his sons that he had left " them gold buried under ground in his vineyard; and they " digged all over the ground, and yet they found none; but by " reason of their stirring and digging the mould about the " roots of their vines, they had a great vintage the following " year." It would, if I do not mistake the matter, be as difficult to explain, how this simile could assist any one to form a correct opinion upon the point in question, as to explain how Bacon

* *Novum Organum*, Lib. i. Aph 127.

Bacon could deliver a wise system of rules for the advancement of physics, without having any just notions of the true nature of philosophical inquiry. The object to which Bacon directed the attention of his followers, was the very object he was desirous they should accomplish,—the regeneration of philosophy by means of a well-regulated use of observation and experiment. The benefits, if any, which accrued to mankind from his directions, were obtained precisely in the way, and were precisely of the kind, which he pointed out and promised. Thus, the case of Æsop's husbandman is so far from furnishing an illustration of Bacon's connection with the advancement of physics, that there is evidently no ground whatever for such a parallel; and the writer who institutes it only proves, that he has altogether mistaken the true bearings of the question. But, before proceeding to state the proofs of this connection, it will be proper to show somewhat more fully, that Bacon's philosophical merit was of the highest kind, and that it was wholly unshared by any other person.

Bacon's grand distinction, then, considered as an improver of physics, lies in this, that he was the first who clearly and fully pointed out the rules and safeguards of right reasoning in physical inquiries. Many other philosophers, both ancient and modern, had referred to observation and experiment in a cursory way, as furnishing the materials of physical knowledge; but no one, before him, had attempted to systematize the true method of discovery; or to prove, that the *Inductive*, is the *only* method by which the genuine office of philosophy can be exercised, and its genuine ends accomplished. It has sometimes been stated, that Galileo was, at least in an equal degree with Bacon, the father of the Inductive Logic; but it would be more correct to say, that his

discoveries

discoveries furnished some fortunate illustrations of its principles. To explain these principles was no object of his; nor does he manifest any great anxiety to recommend their adoption, with a view to the general improvement of science. The Aristotelian disputant, in his celebrated *Dialogues*, is made frequently to appeal to observation and experiment; but the interlocutor through whom GALILEO himself speaks, nowhere takes occasion to distinguish between the flimsy inductions of the Stagyrite, in regard to the subjects in dispute, and those which he himself had instituted; or to hint at the very different complexion which philosophy must assume, according as the one kind or the other is resorted to. Thus, though GALILEO was a great discoverer, it cannot be said that he had any distinction from having taught the principles of the art by which discoveries are made. That distinction belongs wholly to BACON. " No man," says one of the most eminent of our earlier philosophers, " except the incomparable VERULAM, has " had any thoughts of an art for directing the mind in physi- " cal inquiries *."

Some late writers have, however, advanced an opinion, that this distinction does not belong exclusively to any of the moderns †. " It is an error," we are told, " to represent BACON " *as professing his principle of induction to be a discovery.* " *The method of induction, which is the art of discovery, was* " *so far from being unknown to* ARISTOTLE, *that it was often* " *faithfully pursued by that great observer.* What BACON aim- " ed at, he accomplished; which was, not to discover new " principles, but to excite a new spirit, and to render observa- " tion

* HOOKE.—*Posthumous Works*, p. 6.

† See some admirable remarks on this subject, in the *second* volume of Mr STEWART's *Philosophy of the Mind*, Chap. 4. sect. 2.—*On the induction of* ARISTOTLE *compared with that of* BACON.

"tion and experiment the predominant character of philoso-"phy *." It is with considerable diffidence that I dissent from any statement made on the subject of BACON's philosophy by the author of the splendid and instructive essay here referred to. But I must be permitted to express some surprise, that *he* should represent BACON's aims and labours as having been *professedly* limited to the *revival* of a method of discovery which had been well known to, and successfully practised by ARISTOTLE. Nothing can be more certain, than that BACON rests the whole hopes of his philosophy, upon the *novelty* of his logical precepts †; and that he uniformly represents the ancient philosophers, particularly ARISTOTLE, as having been wholly regardless of the inductive method in their physical inquiries. BACON does not, indeed, say, that the ancient philosophers never employed themselves in observing Nature; but he maintains, that there is a wide difference between observation as it was employed by them, and the art of observing for the purposes of philosophical discovery. "Alia enim est ratio "naturalis historiæ, quæ propter se confecta est; alia ejus, quæ "collecta est, ad informandum intellectum in ordine ad con-"dendam philosophiam ‡." BACON does not accuse ARISTOTLE of having always reasoned without any reference to facts; but he contends, that ARISTOTLE has nowhere stated the rules for aiding and regulating the understanding in the process of discovery by means of facts; and that the use which he has made of them in his philosophy, is very different from the use which is made of them in the philosophy of induction. "Ille "enim *prius decreverat*, neque experientiam ad constituenda "decreta

* *Edinburgh Review*, No. liii. p. 186.

† *Novum Organ.* Lib. i. Aph. 82. 95. 97. 125.

‡ Ibid. Lib. i. Aph. 98.

"decreta et axiomata rite consuluit; sed postquam pro arbitrio suo decrevisset, experientiam ad sua placita tortam circumducit, et captivam *." It should always be recollected, that BACON's call was not merely for observation and experiment; but for observation and experiment conducted according to certain forms and rules; which forms and rules were first delineated by him, and constitute the body of the Inductive Logic. There may be nothing in this logic that can be called a *discovery* in the strict sense of the word; but the statement of its precepts, was certainly a grand and important step towards the advancement of genuine science.

It would require a complete analysis of the *Novum Organum* to furnish an adequate idea of the value of BACON's services in this important department of philosophy; but the *fundamental* rules of his method may be comprehended in a few sentences. They seem all to be founded upon the following principles: first, That it is the business of philosophy to discover the laws or causes that operate in Nature, in order thereby to explain appearances, and produce new effects †: next, That we are incapable of discovering these laws or causes in any other way than by attending to the circumstances in which they operate: and, lastly, That the mind is naturally disposed to run into general conclusions, and to form systems, before

* *Novum Organum*, Lib. i. Aph. 63.

† *Novum Organ.* Lib. i. Aph. 117. Throughout the whole of the first book, the object of science is represented to be the discovery of *Axioms;* by which term BACON evidently means those general laws or truths which form the basis of our physical reasonings. NEWTON, as Mr STEWART observes, has, after BACON's example, applied the term *Axiom* to the laws of motion, and to the statement of certain general truths in Catoptrics and Dioptrics. See *Philosophy of the Mind*, vol. ii. Chap. 4. They who are engaged in the study of the *Novum Organum*, will derive much valuable information and assistance from the perusal of this part of Mr STEWART's work.

fore having made all the inquiries necessary to truth. In conformity with these principles, he shows, that all sound philosophy must proceed *from* facts; that the facts in every case must be carefully collected and compared; and that in all our reasonings about them, the natural tendency of the mind to generalize must be carefully repressed. The *spurious* method of induction is that which proceeds suddenly from particulars scantily collected or ill examined to the most general conclusions. The *true* method is that which lays a wide basis in observations and experiments, and which generalizes slowly; advancing gradually from particulars to generals, from what is less general to what is more general, till the inquiry ends in truths that appear to be universal *.

Nothing could be more encouraging or animating, than BACON's recommendations of this plan of inquiry. Though he held that the noblest end of philosophy is the discovery of truth †, he taught that there is a correspondence between this and another end, also of great dignity,—the improvement of the outward accommodations of human life. He showed, that, when the principles of science should really be derived from the knowledge of Nature, their discovery would prove beneficial to man, as well in respect to the increase of his *power* as of his *knowledge;* because the principles so discovered would lead to new inventions in the useful arts, and to new rules for the improvement of all the operative parts of knowledge. He endeavoured to stimulate the spirit of inquiry, by representing the field of scientific discovery, as yet almost wholly uncul-

* *Nov. Organ.* Lib. i. Aph. 100, 101, 102, 103, 104, 105.

† Ibid. Aph. 124. 129. He takes some pains here and elsewhere to guard against the supposition that he valued science only as it was calculated to augment the outward accommodations of life.

tivated, and by assurances that it only required to be cultivated with attention to his rules, in order to yield an endless increase of knowledge and of inventions. "Let it be believed," says he, "and appeal thereof made to time, *with renunciation, nevertheless, to all the vain and abusing promises of the Alchymists, and such like credulous and fantastical sects*, that the new found world of land was not greater addition to the old, than there remaineth at this day a world of *Inventions* and *Sciences* unknown, having respect to those that are known, with this difference, that the ancient regions of knowledge will seem as barbarous compared to the new, as the new regions of people seem barbarous compared to many of the old *." It is in these confident anticipations of the future triumphs of science, so often repeated as encouragements to its faithful prosecution, that we more particularly perceive the grandeur and reach of his views. His predictions of improvement were not the vague or casual surmises of a happy enthusiasm; they were evidently grounded upon an enlightened conviction, that the business of philosophy had hitherto been mistaken, and that her labours would prosper, when they should be employed with constancy and skill upon their legitimate objects.

Is it not unreasonable to doubt the utility of a system of logical instructions, in which the true art of discovery was, for the first time, explained? These instructions were offered at a period in every respect opportune. There was a growing disposition to revolt against the Schools, and a wise leader was wanted to raise the true standard of reform, and to give a salutary direction to the pursuits of those who should emancipate themselves from their authority. The improvement

* *Of the Interpretation of Nature*, Chap. i.—Works, vol. i. p. 376. 4to edit.

ment of some branches of physics was already in part begun; but there was no general agreement as to the rules of inquiry. The truths which BACON taught are now, it is true, known, and their authority acknowledged by all; but this was far from being the case in the early part of the seventeenth century. One of the most intelligent of his friends, Sir THOMAS BODLEY, to whose judgment he submitted an early sketch of his plan, appears to have been wholly unable to distinguish between the loose procedure of the Empirics and that regulated procedure which it recommends. "As for that," says he, "which you inculcate of a knowledge more excellent "than now is among us, which experience might produce, "if we would but essay to extract it out of Nature by particu-"lar probations; it cannot, in reason, be otherwise thought, "but that *there are infinite numbers which embrace the course "that you propose, with all the diligence and care that ability can "perform.* I stand well assured," he concludes, "that for the "tenor and subject of your main discourse, you will not be "able to impannel a substantial jury in any university, that "will give up a verdict to acquit you of error*." But that which places the importance of BACON's logical instructions in the strongest light, is the fact, that one of the most celebrated of his contemporaries, who also professed himself a reformer of philosophy, employed the better part of his life, in teaching doctrines as diametrically opposite in principle as in tendency. This was DESCARTES. "Never," says an eloquent philosopher, "did two men, gifted with such genius, recommend paths of in-"quiry so widely different. DESCARTES aspired to deduce an ex-"planation of the whole system of things by reasoning *a priori* "upon

* *Sir THOMAS BODLEY's Letter to Sir FRANCIS BACON about his* COGITATA ET VISA.—BACON's Works, vol. iii. p. 242, 243, 244.

" upon assumed principles: BACON, on the contrary, held, that " it was necessary to observe Nature thoroughly before at- " tempting to explain her ways; that we must ascend to prin- " ciples through the medium of facts; and that our conclu- " sions must be warranted by what we observe. DESCARTES " reasoned about the World, as if the laws which govern it had " not yet been established, as if every thing were still to " create. BACON considered it as a vast edifice, which it was " necessary to view in all directions, to explore through all its " recesses and windings, before any conjecture even, could be " safely formed as to the principles of its construction, or " the foundations on which it rests. Thus, the philosophy of " BACON, by recommending the careful observation of Nature, " still continues to be followed, whilst that of DESCARTES, " whose essence lay in hypothesis, has wholly disappeared *." Nor was DESCARTES, I may add, ignorant of what BACON had taught as to the principles of philosophizing. It appears, on the contrary, from his correspondence, that he was well acquainted with BACON's writings; and, in one of his letters, he seems to admit, that provided the Experimental were the true Method, there was nothing that could be added to increase the utility of BACON's precepts †.

Having made these remarks, with a view to point out, in a general way, the nature and importance of those helps and encouragements which BACON's writings furnished to physical inquiry, I am next to endeavour to show, that the subsequent progress of physical knowledge was greatly accelerated by the effects which they produced. And here I beg to observe, that

* BAILLY.—*Histoire de l'Astronomie Moderne*, tom. ii. liv. 4. § 2.

† *Lettres de* M. DESCARTES, tom. iv. p. 201, Paris edit. 1724.

that I have no argument with those who hold, that the reformation of philosophy by the adoption of the Inductive Method would have taken place in time, though BACON had never written; any more than with those who hold, that physical science owes nothing to him, on the score of any discovery of importance made by himself, or deduced by others from his suggestions. I have before stated, that this reformation was already in progress, and that the Inductive Method had been happily *exemplified* in the discoveries of some of his contemporaries. The proposition here maintained is, that BACON did *more* to forward its general adoption than any other person; and this,—because his writings contributed more than the labours of any other individual, to complete the abandonment of the scholastic methods and systems,—to generate a relish for experimental inquiries,—and to imbue the minds of the ingenious with the views and principles requisite to conduct these inquiries with success. The way to prove that BACON's writings were powerful agents in the advancement of physical knowledge, is to prove that they produced these effects; and the proof that such effects were actually produced by them, must necessarily be derived from the testimony of those who early experienced, or became otherwise acquainted with their operation.

The reputation which BACON had acquired from his *Essays*, a work early translated into various foreign languages; his splendid talents as an orator, and his prominent place in public life,—were circumstances strongly calculated to attract the curiosity of the learned world to his Philosophical Writings; and from some of which, he derived advantages in regard to their circulation, not possessed in that age by ordinary men. These writings accordingly appear to have been early read by the learned at home, and early transmitted to the learned abroad; and

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it farther appears, that the important truths which they disclosed did not remain long unperceived, or barren of consequences. "Dr Collins, Provost of King's College, Cambridge, "a man of no vulgar wit, affirmed unto me," says Bacon's Chaplain, Dr Rawley, "that after reading the *Advancement of* "*Learning*, he found himself in a case to begin his studies "anew, and that he had lost all the time of his studying be-"fore *." Of his more recondite work, his distinguished contemporary Ben Johnson speaks as follows: "The *Novum* "*Organum* is not penetrated or understood by superficial men, "who cannot get beyond *Nominals, but it really openeth all* "*defects of knowledge whatsoever;* and is a book

"Qui longum noto Scriptori proroget ævum *."

Sir Henry Wotton, another of the most eminent men of that day, thus warmly expresses his opinion of its merits: "I have "received," says he, in a letter to Bacon, written from Ger-"many, three copies of that work, wherewith your Lordship "hath done *a great and everlasting benefit to all the children of* "*Nature*, and to Nature herself in her utmost extent and lati-"tude, who never before had so true an Interpreter, or so inward "a Secretary of her Cabinet †." In this letter, Sir Henry gives an interesting account of an accidental meeting which he had lately had with the celebrated Kepler, in Upper Austria; to whom, he adds, he was about to send one of his copies of the *Novum Organum*, for the honour of England. It is not surprising, that a writer who entertained such sentiments in regard to the importance

* *Life of Bacon*, prefixed to Rawley's *Resuscitatio, or bringing to light several pieces of the Works of Lord Bacon.*

† Ben Jonson's *Discoveries.—Works*, vol. vii. p. 100. Whalley's edition.

‡ *Reliquiæ Wottonianæ*, p. 299. 3d edition.

importance of BACON's Philosophy, should have been led to predict the speedy downfal of that of the Schools. "Sir HENRY "WOTTON," says Dr BEALE, in a letter to Mr BOYLE, written about forty years after this period, "would often please him-"self in lashing the Schoolmen; and would often declare it "as a serious prediction, that in *this age* their reputation "would yield to more solid philosophy." Dr BEALE adds, that he had himself been weaned from the errors of the Schools, by the early perusal of BACON's philosophical writings *.

In a letter to King James, written about the period of the publication of the *Novum Organum*, BACON states, that the *Advancement of Learning*, had been very favourably received in the Universities; and he thence draws the conclusion, that the *Novum Organum* would also be acceptable to them; because, says he, "it is only the same argument "sunk deeper †." In an address presented to him by the University of Oxford, in the year 1623, he is represented as a "mighty HERCULES, who had by his own hand "greatly advanced those pillars in the learned world, which, "by the rest of the world, were supposed immoveable ‡;" and this piece of homage, it is to be observed, was offered at a time when all motives to interested adulation had been done away by his lamentable fall. These facts seem to evince, that BACON's writings had early made a strong impression, even in quarters where favourable effects were not likely to be speedily produced; and accordingly, we are informed by Bishop

* BOYLE's *Works*, vol. vi. p. 355.

† BACON's *Works*, vol. iii. p. 584.

‡ TENNISON's *Baconiana, or certain Genuine Remains of* Sir FRANCIS BACON, p. 206.

Bishop Sprat, that when some of those ingenious men who afterwards assisted in forming the *Royal Society*, began, about the end of the Civil War, to establish a weekly meeting at Oxford for philosophical discussion, they found, that the new spirit of "free inquiry" had already made considerable progress among the members of the University *.

When one of Bacon's friends asked him, Whether he thought the Churchmen likely to oppose his intended reformation of philosophy, his answer was—"I have no occasion to meet "them in my way, except it be, as they will needs confede-"rate with Aristotle, who, you know, is intemperately "magnified by the School-Divines †." We are told by Osborn, a contemporary observer, that the "School-Divines" did endeavour to cry down his philosophical writings, by representing them as favouring *atheism* ‡. This was their usual mode of warfare when the established tenets of the Schools were attacked by any formidable opponent. The Aristotelians of all descriptions,

* Sprat's *History of the Royal Society*, p. 53; also p. 328.——This spirit appears to have made still greater progress at Cambridge. Glanvill, who became a student of Exeter College, Oxford, in 1652, "lamented," says Anthony Wood, "that his friends did not send him to Cambridge; because, he used to say, that the *new philosophy, and the art of philosophizing*, were more cultivated there, than here at Oxford."—*Athen. Oxon.* vol. ii. p. 662. —"After the way of free-thinking," says Baker, "had been laid open by Lord Bacon, *it was soon after greedily followed.*" See his *Reflections on Learning.* This work was first published in 1699. The author, who was a Fellow of St John's College, Cambridge, was deeply read in the history of that University. His extensive collections upon that subject are deposited in the British Museum.

† Bacon's Letters to Sir *Toby Matthew*, in his *Works*, vol. iii. p. 247, 257.

‡ Introduction to Osborn's *Miscellany of Essays, Paradoxes, and Discourses.*

descriptions, appear to have early manifested a decided hostility to his philosophy; and their criticisms are sometimes expressed in a way which plainly testifies that it had made considerable progress. The examination of his *Sylva Sylvarum*, by ALEXANDER ROSS, now much better known by BUTLER's sarcastic allusion in the poem of *Hudibras*, than by any of his own multifarious productions, furnishes a curious example. It was published in the year 1652, that is, about twenty-five years after BACON's death. "I have," says he, "cursorily run over my "Lord BACON's *New Philosophy*, and find that philosophy is "like wine, the older the better. For, whereas ARISTOTLE "had, with infinite pains and industry, and not without sin-"gular dexterity, reduced all entities into certain heads, and "placed them in ten classes or predicaments to avoid confu-"sion, and that we might, with the more facility, find out the "true genus and difference of things; which Aristotelian "way hath been received and approved by all Universities, "and the wise men since his time in all ages, as being the "most consonant to reason: yet these *New Philosophers*, as if "they were wiser than all the world besides, have, like fantastic "travellers, *left the old beaten path, to find out ways unknown*, "and have reduced his comely order into chaos; jumbling "the predicaments so together, that their scholars can never "find out the true genus of things." The examples which he adduces in illustration of this disorder, are in fact proofs of the growing taste for experimental inquiry; and it is clear from the context of the whole passage, that BACON was considered by the Aristotelians as having been its chief promoter. "Sometimes," he continues, "these *New Philosophers* tell us, "that heat, cold, &c. are spirits, consequently substances; "sometimes, again, they will have them to be qualities; some-"times to be motions and actions. Thus, Proteus-like, they

 "turn

"turn themselves into all shapes, so that we know not in what "predicament to put their heat, or what genus to give it *."

That *New Philosophy*, which had already produced so much embarrassment among the followers of ARISTOTLE, had already also led to the formation of a Philosophical Society, destined, at no distant day, to realize, in some sort, one of BACON'S favourite projects. In his letter to King James, written on the publication of the *Novum Organum*, he states, that his chief object in publishing the work, before completing it according to his original plan, was, to try to procure help towards compiling an "experimental history of Nature †." He more than once alludes, in the work itself, to the great things that might be accomplished in philosophical inquiries, by a conjunction of labours; and in a romantic piece, called the *New Atlantis*, he gives an account of a feigned *College* or *Society*, magnificently endowed, and whose business was the improvement of all the departments of physical knowledge. To this College he gives the name of *Solomon's House*. The intention of this piece evidently was, to exhibit a grand and alluring representation of the advantages that might be derived from the co-operation of numbers in scientific pursuits, and of the renown that a Prince might acquire by forming an establishment directed to such purposes. These views and schemes were not forgotten by his followers. In the year 1645, a Society was formed in London, for the purpose of discussing subjects connected with Natural Philosophy, at stated weekly meetings; and the name first given to this Society appears to have been that of the *Philosophical College* ‡. Some

* Ross's *Arcana Microcosmi*, or the hid secrets of Man's body discovered; with a refutation of Lord BACON'S *Natural History*, p. 263, 264.

† BACON'S *Works*, vol. iii p. 584.

‡ See BOYLE'S *Life*, prefixed to his Works, p. 34. This Society was sometimes called the *Invisible College*.—Ibid. p 40. 42.

Some of its members being soon thereafter appointed to Professorships in the University of Oxford, a similar Society was established by them in that place. In the year 1659, the principal members of the Oxford branch having returned to London, the two Societies were united; and having, on the Restoration, extended their views to the obtaining a public establishment, they, in 1662, succeeded in accomplishing that object, by being erected into a corporate body, under the title of the *Royal Society*.

There can be no doubt whatever, of the influence of BACON's suggestions, as to the utility of such an institution, upon the minds of those who planned the establishment of this illustrious Society. Its earliest panegyrists and historians bear testimony to this fact. "*Solomon's House*, in the *New Atlantis*, was "a prophetic scheme of the Royal Society." These are the words of GLANVILL, in his address to that body, prefixed to his *Scepsis Scientifica*, published in 1665*. Bishop SPRAT, whose *History*

* The *Scepsis Scientifica* is a republication, with some additions, of GLANVILL's first philosophical work, *The Vanity of Dogmatizing*, published in 1661. The 20th chapter of this work contains a very distinct statement of the important doctrine so often ascribed to Mr HUME,—that we never *perceive* causation in the succession of physical events; a doctrine which fixes the object of physical science to be, not the investigation of the efficient causes of phenomena, but of the general laws by which they are regulated; and for which statement of its legitimate objects, it is always to be remembered, that *physics* is indebted to *metaphysics*. The Aristotelians were provoked by the free spirit of inquiry, and disregard of the authority of their Master, which this work disclosed; and an answer to it appeared in 1663, in a book entitled *Sciri, sive Sceptices et Scepticorum à jure disputationis exclusio*. The author was THOMAS ALBIUS, (WHITE), a secular priest of the Romish Church, and a noted Aristotelian. "HOBBES," says A. WOOD, "had a great respect for WHITE, and when he lived in Westminster, he would often visit him, and he HOBBES; but they seldom parted in cool blood: for they would wrangle, squabble and scold about philosophical matters like young sophisters, though either of them was eighty years of age. HOBBES

History of the Society, published in 1667, received its public sanction, expresses himself as follows: "The Royal Society "was a work well becoming the largeness of BACON's wit to "devise, and the greatness of CLARENDON's prudence to esta-"blish *." SPRAT also informs us, that the Tract published in 1661, by COWLEY, entitled, *A Proposition for the Advancement of Experimental Philosophy*, "very much hastened the contri-"vance of the platform of the Royal Society;" and this Tract bears internal evidence that its author's views were originally derived from the *New Atlantis*.

But it is of more importance to show, that the philosophical spirit which actuated the founders of this institution, was chiefly owing to the effects produced by BACON's writings. And here, again, I must appeal, in the first place, to the testimony of those to whom we are indebted for all that we know of its early history. The fullest account of its origin is given by the celebrated mathematician Dr JOHN WALLIS, who was one of those who instituted the weekly meetings begun to be held in London in 1645; and *his* narrative distinctly points to BACON, as having given a beginning to the taste for experimental science in England. "Our business," says he, "was to discourse and consi-"der of things appertaining to what hath been called the *New* "*Philosophy*, which, from the times of GALILEO, and Lord "VERULAM, hath been much cultivated abroad, as well as with

HOBBES being obstinate, and not able to bear contradiction, those who were sometimes present at their wrangling disputes, held that the laurel was carried away by WHITE."—*Athenæ Oxon.* vol. ii. p 665. The *Scepsis Scientifica* has, appended to it, a reply to the animadversions contained in WHITE's *Sciri* upon the *Vanity of Dogmatizing*.

* *History of the Royal Society*, p. 144. Copies of this work were sent, by the Society, to foreign Princes, and other eminent persons abroad, in order to furnish them with an authentic account of its history. See Dr BIRCH's *History of the Royal Society*, vol. ii. p. 207.

" with us in England *." SPRAT always speaks of Lord BACON, as the founder of that experimental school, which came to be embodied in the institution whose history he wrote †; and the testimony of Mr OLDENBURG, its first Secretary, though a foreigner, is equally explicit. " The enrich-" ment of the storehouse of *Natural Philosophy*, was a work," says he, " *begun by the single care and conduct of the ex-" cellent Lord* VERULAM, and is now prosecuted by the joint " undertakings of the Royal Society ‡." GLANVILL, whose zeal in defending this establishment, against the attacks of its enemies, well entitles him to respectful notice in the history of philosophy, makes frequent acknowledgements to the same purpose. The following passage contained in the work which he wrote in its defence, and which was published in 1668, under the title of *Plus ultra, or, the Advancement of Knowledge since the days of* ARISTOTLE, is too remarkable to be omitted on the present occasion. " The philosophy that must signify either for light " or use, must not be the work of the mind turned in upon itself, " and only conversing with its own ideas; but must be rai-" sed from the observations and applications of sense, and [illegible] " take

* See his *Account of his own Life*, in a Letter published in the Appendix to HEARNE's *Preface* to LANGTOFT's *Chronicle*, Number IX.

† See particularly p. 35. *History of the Royal Society*.

‡ *Philosophical Transactions*, No. 22. p. 391.——Mr OLDENBURG frequently alludes to BACON as the *chief* forwarder of experimental philosophy. " When our renowned Lord BACON had demonstrated the methods for a perfect restoration of all parts of real knowledge, *the success became on a sudden stupendous*, and effective philosophy began to sparkle, and even to flow into beams of bright shining light all over the world."—Pref. to *Philosophical Transactions* for 1672. —" Many of the chief Unversities in Christendom have formed themselves into philosophical societies, and have largely contributed their aids to advance Lord BACON's design for the instauration of arts and sciences."—Pref. to *Philosophical Transactions* for 1677.

"take its accounts from things as they are in the sensible "world. The illustrious Lord BACON hath noted it as the "chief cause of the unfruitfulness of the former methods of "knowledge, that they were but the exercises of the mind "making conclusions, and spinning out notions from its own "native store; from which way of proceeding nothing but dis- "pute could be expected *.—He therefore proposed another "method, which was, to reform and enlarge knowledge by ob- "servation and experiment; to examine and record particu- "lars; and to rise by degrees of induction to general proposi- "tions; and from them to take observation for new inquiries; "so that nature being known, may be mastered, and used in "the service of human life. This was a mighty design, "groundedly laid, and happily recommended by the glorious "author; but to the carrying it on, it was necessary there "should be many heads and many hands, and those form- "ed into an assembly that might inter-communicate their "trials and observations. This the great man desired, and "formed a Society of experimenters in a romantic model; "but he could do no more; his time was not ripe for such per- "formances. *These things, therefore, were considered by the* "*later Virtuosi, who several of them combined together, and* "*set themselves to work upon his grand design* †."

Similar

* GLANVILL's *Plus Ultra*, p. 52.

† Ibid. p. 87, 88.—There are some who would fain persuade us, that the taste for experimental philosophy was introduced into England from the Continent, and that the first idea of the Royal Society was copied from similar associations abroad. This, certainly, was not the language of the founders and early historians of that Society. It is curious to remark, that while some of our own writers ascribe its origin, and the philosophical spirit which gave it birth, to foreign excitements, there are, on the other hand, foreign writers who trace the Academies of the Continent to the effects produced by the writings

of

Similar testimonies occur in many other publications of that day; in the more obscure as well as the more noted. Indeed, there is no room whatever for doubt, that BACON was generally considered as the *chief* promoter of genuine physics, at a period when the erection of the Royal Society, was of course likely to bring forward the name of any individual, whose labours had contributed, in a remarkable degree, to foster the growth of physical science. COWLEY, surely, will not be rejected as an evidence of the general sentiment, merely because he has recorded his testimony in verse. He was, as already mentioned, a zealous advocate for a public institution, directed to the purposes of experimental philosophy; and, on the establishment of the Royal Society, he addressed to it that celebrated Ode in which he represents BACON as its Legislator. DR HENRY POWER calls BACON "the Patriarch of experimental "philosophy;" in a work published in 1664, in which he de-

tails

of BACON. The following passage is extracted from a very learned *History* of one of the earliest of these Academies.—"Sed, quæ superest dicenda, supremam, et, ut nobis videtur, proximam condendæ Academiæ enarrabimus occasionem. Scilicet postquam, ineunte circiter priori seculo, non inter Britannos solum, sed universi quoque orbis incolas, immortalitati commendatissimus, FRANCISCUS BACO de Verulamio, supremus regni Britannici Cancellarius, variis iisque ad sapientiæ normam elucubratissimis scriptis, utilissima emendandæ atque instaurandæ historiæ naturalis dedisset consilia, et absolutissimis rationibus firmasset: *non Angli modo haud incassum se moneri atque excitari passi sunt, sed exteræ quoque gentes*, imprimis Galli Italique, sanioris consilii patientes, tanta contentione cum qualibuscunque scientiis generatim, tum præcipue rerum naturalium studio animum intenderunt, adeo, ut ex illo tempore visi sint homines nihil, vel remotissimis naturæ visceribus abstrusum, quod non captis ex BACONIS mente experimentis curiosius rimarentur, relicturi. *Atque hic ardor, hæc studia magnam quoque partem condiderunt Academiarum Societatumque hactenus memoratarum.*" BUCHNERI, *Academ. Naturæ Curiosor. Hist.* cap. i. § 7.

tails the discoveries of GALILEO, TORRICELLI, and PASCAL *. "It is certain," says Mr HAVERS, in the preface to a work also published in that year, entitled, *Philosophical Conferences*, "that Lord BACON's way of experiment, as now prosecuted "by sundry English gentlemen, affords more probabilities of "glorious and profitable fruits, than the attempts of any other "age or nation whatsoever †." Dr JOSHUA CHILDREY, in the introduction to his *Natural Rarities of England*, a book of the same period, and which gave rise to a new class of publications in Natural History, states, that he had given it the title of *Britannia Baconica*, in order to indicate its connection with those studies which BACON had originated ‡. ANTHONY WOOD has preserved a letter from the same person to Mr OLDENBURG, Secretary of the Royal Society, in which he says, that he had long been engaged in the philosophical inquiries "which form the business of that body; in consequence of having fallen in love with "Lord BACON's Philosophy as early as the year 1646 ||." Mr EVELYN, one of the most active and respected of the early members of the Society, has, in several of his works, alluded to the beneficial effects produced by BACON's philosophical writings. In the introduction to his *Sylva*, which work he published in 1664, at the request of the Royal Society, he takes occasion to state the philosophical principles by which it professed to be guided, in terms which clearly point to the quarter from whence they were derived. "They are not hasty," says he, "in pronouncing from a single or incompetent number of "experiments;

* POWER's *Experimental Philosphy*, p. 82.

† *Philosophical Conferences*, translated from the French, by G. HAVERS, in two volumes folio.

‡ *Britannia Baconica, or the Natural Rarities of England*, 1661, 8vo. "From this work," says A. WOOD, "Dr PLOT took the hint of his *Natural History* of Oxfordshire."

|| *Athenæ Oxonienses*, vol. ii. p. 468.

"experiments; but after the most diligent scrutiny, and by "degrees, and by wary inductions faithfully made, they re-"cord the truth and event of trials, and transmit them to po-"sterity. They resort not immediately to general proposi-"tions upon every specious appearance; but seek light and in-"formation from particulars, that they may gradually advance "to general rules and maxims." In an after work, he speaks of BACON's services in the following expressive terms: "By "standing up against the Dogmatists, he emancipated and set "free philosophy; which had long been a miserable cap-"tive; *and which hath ever since made conquests in the ter-"ritories of Nature* *."

It was about this period, that Mr BOYLE was honoured with the appellation of the second BACON †, in compliment to his exertions to advance the knowledge of experimental physics; and there can be no doubt, that his discoveries and exertions did contribute essentially to establish the credit of the English School. Neither can there be any doubt, as to the influence of BACON's writings in determining the nature and objects of his philosophical pursuits. This is admitted, or implied, in many parts of his works ‡. It is clear, indeed, that he was considered by his contemporaries as a marked disciple of BACON. "You have," says Dr BEALE, in one of his letters to him, upon the subject of his discoveries, "*particularised,* "*explicated, and exemplified, those fair encouragements, and* "*affectionate directions, which Lord* BACON *in a wide generali-*

* EVELYN's *Numismata.*

† See GLANVILL's *Plus Ultra*, p. 57.

‡ BOYLE's *Works*, vol. i. p. 305, 6.; vol. ii. p. 472.; vol. iii. p. 422.; vol. iv. p. 59, 246.; vol. v. p. 567.

"*ty proposed* *." In another letter, to Mr HARTLIB, who like himself was an early and zealous promoter of the Royal Society, Dr BEALE thus rapturously expresses his feeling of the pleasure which BOYLE's experimental labours were calculated to afford to the followers of Lord BACON. "To those that have "been tired and wearied, as I have been in the several ways "of former philosophers; to those who have condescended to "take deep notice of the insufficiency of conjectures, and un-"grounded ratiocinations, and who have submitted their pa-"tience to the *severity of Lord* BACON's *inquisitions*, here are "offered such pleasing refreshments, as give us the relish of "that Virgilian simplicity, which was so highly admired by "SCALIGER in these verses:

"Tale tuum carmen nobis, divine poëta,
"Quale sopor fessis in gramine, quale per æstum
"Dulcis aquæ saliente sitim restinguere rivo †."

They who have overlooked or disregarded the proofs of the connection between what BACON enjoined, and BOYLE performed, are not likely to have recognized any traces of the lights held out by the former, in the philosophy of NEWTON. Yet it appears undeniable, that the latter was guided by principles which BACON alone had taught; and that his philosophy derives its imperishable character from his rigid adherence to them. To begin with the examination and comparison of phenomena in order to rise to the knowledge of general truths, and to proceed

* BOYLE's Works, vol. vi. p. 405.

† This letter is printed in the *Life* of BOYLE, prefixed to his works, p. 63.—"Dr BEALE was elected a Fellow of the Royal Society in 1662. Several of his papers are printed in the *Transactions*. He was a man of excellent parts, and great public spirit; and the character which his friend Mr HARTLIB gave of him was, that there was no man in the island who could be made more universally useful."—BIRCH's *Hist. of the Royal Society*, vol. iv. p. 235.

ceed gradually from truth to truth, till we reach the most general that can be discovered,—these are the principles of philosophizing which BACON unfolded, and which NEWTON has, in the most emphatic terms, embodied with his discoveries. "*Quel témoignage*," exclaims an eminent French philosopher, "*rendu par le génie inventeur au génie des méthodes* * !" Such, indeed, was the connection between the logic of the *Novum Organum*, and the philosophy of the *Principia*, that it was only where the one was followed, that the other prevailed. The sublime Geometry of the *Principia*, says MACLAURIN, was admired by all, but it was only among minds trained by BACON'S precepts that it found a ready reception for its Philosophy †.

To these proofs of the influence of BACON'S precepts and exhortations, reflected in the acknowledgments, the views, and the discoveries of the early founders of the English School of Experimental Philosophy, I have yet to add those which are furnished by the writings of its opponents and detractors. The public countenance given to that School by the erection of the Royal Society, early excited an extraordinary degree of jealousy on the part of the Universities; and a keen spirit of opposition among the remaining supporters of the Aristotelian philosophy. SPRAT accordingly found it necessary, in his *History* of the Society, to employ a long argument to prove, that this new establishment would be attended with no

* DEGERANDO—*Histoire comparée des Systemes de Philosophie*, tom. i. p. 396.—The introduction to Dr PEMBERTON'S *Account of Newton's Discoveries*, a work, "the greater part of which was read and approved," as we are told in the preface, by NEWTON himself, contains a summary of the doctrines of the *Novum Organum*; and its author is represented as the first who taught those rules of philosophizing which NEWTON followed, and which his discoveries so nobly confirmed.

† MACLAURIN'S *Account of Newton's Discoveries*, p. 59, 60.

no bad consequences either to religion, or to the existing seminaries of knowledge. GLANVILL was obliged to enter into a serious refutation of an assertion, that "ARISTOTLE had had "more advantages for knowledge than the Royal Society, either had, or could have *." The panegyrics which these writers bestowed upon the Institution, and upon Lord BACON as its Master, appear to have filled the followers of ARISTOTLE with a still more envenomed hate to both. The most forward of their champions was Dr HENRY STUBBE, who, after studying at Oxford, had served for some time in Scotland with the Army of the Parliament; but having on the Restoration made his peace with the Government, he was appointed King's Physician for the Island of Jamaica, from whence he had lately returned, to practise in his own country. He was, according to ANTHONY WOOD, "the most noted Latinist and Grecian of "his age, and a singular Mathematician;" but he seems to have been as deficient in judgment as he was violent in temper; which last defect, his biographer in great simplicity ascribes to his "carrot-coloured hair †." His publications against the Royal Society, and the whole body of experimentalists, were numerous,

* "I desire the reader to know, that after Mr JOSEPH GLANVILL had written certain things against ARISTOTLE, it was the desire of some scholars, that ROBERT CROSSE, a noted philosopher after the ancient way, should be brought acquainted with him. In 1667, GLANVILL was therefore conducted to his house, where CROSSE did in a sufficient manner vindicate ARISTOTLE; and did plentifully declaim against the proceedings of the Royal Society. GLANVILL being surprised, he did not then much oppose him; but afterwards he did, to the purpose; especially against this hypothesis of CROSSE, *that ARISTOTLE had more advantages for knowledge than the Royal Society, or all the present age had or could have, because he did* totam peragrare Asiam."—*Athenæ Oxonienses*, vol ii. p. 753. See the account which GLANVILL himself gives of this conference, *Plus Ultra*, p. 4, 5.

† WOOD's *Athen. Oxon.* vol. ii. p. 562, 563.

rous, and all of them replete with misapplied learning, and vehement abuse. The course of his reasoning is not a little curious. "I have so small a regard," says he, "for deep and "subtle inquiries into natural philosophy, that could physic "be unconcerned, could religion remain unshaken, could edu-"cation be carried on happily, I should not intermeddle; but if "we look *de facto* upon those experimental philosophers, and "judge how little they are fitted for trusts and managements "of business, by their so famed *mechanical education*, we must "rise as high in our resentments as the concerns of the pre-"sent age and of posterity can animate us." The grounds which he more particularly assigns for entertaining these "high "resentments" against the experimentalists, are, first, their neglect and contempt of the Aristotelian logic; "that art," says he, "by which the prudent are discriminated from fools; "which informs us of the validity of consequences, and the "probability of arguments, and which forms statesmen, di-"vines, physicians, and lawyers." In the next place, he contends, that the innovating spirit of their philosophy would lead to dangerous revolutions. "In such times," says he, "as I "thought it our interest to subvert the monarchy of England, "and the repute of the clergy, I was passionately addicted to "this *new philosophy*; for I did not question but the autho-"rity of all antiquity in *spiritual* affairs would vanish, when it "appeared how much churchmen were mistaken in the com-"mon occurrences and histories of nature. How rational this "opinion of mine was, and how it is verified in these days, let "the Hierarchy and Universities judge *."

With

* STUBBE's *Legends no Histories; or, a specimen of some animadversions upon the History of the Royal Society.* Pref. 4to. Lond. 1670.

With such views of the new philosophy, this infatuated Aristotelian could not but wish to decry the authority of any one who was more particularly considered as its author. That he himself looked upon the experimentalists of that day as the disciples of BACON, is sufficiently evident from this, that his common mode of designating them, is to call them in derision "*a Bacon-faced generation**." To abuse Lord BACON, and to depreciate his philosophical character, are accordingly his favourite topics. Nor does he leave us in any doubt as to the cause of his enmity. It was, as he expressly tells us, "*because the repute* "*of Lord* BACON *was great in that age* †;" and because "*the* "*Royal Society pretended to tread in his footsteps* ‡." He allows that BACON was a wise and eloquent man; but with respect to his censures of the philosophy and methods of the ancients, there, says he, he was insufferably in error. "Who "knows not," he asks, "how *Herbary* had been improved by "THEOPHRASTUS, DIOSCORIDES, the Arabians, and other Peri- "patetics? who can deny that *Physic*, in every part of it was "improved, by GALEN and others, before the Lord BACON "ever sucked? and what accessionals had not *Chemistry* recei- "ved by the cultivation of the Aristotelians, before his *House* "*of Solomon* was dreamed of? Let us, therefore, not be con- "cluded by the aphorisms of this Lord. Let his *insulse ad-* "*herents* buy some salt, and make use of more than one grain "when they read him; and let us believe better of the an- "cients, than that their methods of science were so unfruit- "ful ||." It was the confident belief of this misguided man, that

* STUBBE's *Epistolary Discourse concerning Phlebotomy*, passim. 4to. Lond. 1671.

† *Lord BACON's Relation of the Sweating Sickness examined*, p. 2. 4to. Lond. 1671.

‡ *Legends no Histories*, p. 29.

|| *Lord BACON's Relation of the Sweating Sickness examined*, Pref. p. 5.

that BACON's fame was wholly owing to the false notions of philosophy then entertained; and that it could not fail to fade with the recurrence of sounder views. "The Lord BACON," says he, "is like great piles; when the sun is not high, they "cast an extraordinary shadow over the earth, which lesseneth as the sun grows vertical *." How vain the prophecy involved in this uncouth simile! The fame of BACON has brightened as Science has advanced, every new discovery bringing a fresh proof of that transcendent sagacity which enabled him so unerringly to plan and predict the indefinite enlargement of her Empire.

The preceding illustrations of the influence of BACON's writings, are confined to the effects which they produced in England. It remains to be inquired, Whether they were productive, in any degree, of any similar effects, in the other countries of Europe? It is the opinion of some, who are far from being otherwise sceptical as to their influence, that these writings were, for a long period, but little known upon the Continent; and consequently, that all their effects, of a *direct* kind, were limited to England. This opinion has been lately avowed by one of the most enlightened and ardent of BACON's admirers; one whose extensive knowledge in regard to the history of learning, I shall hardly, I trust, be suspected of any intention to bring into doubt, by dissenting from his statements on this particular question.

"That the works of BACON," says Mr STEWART, "were but "little read in France till after the publication of D'ALEM-

* STUBBE's *Legends no Histories*, p. 28.

"BERT's *Preliminary Discourse* to the *Encyclopedie*, is, I believe, an unquestionable fact; *not* that it necessarily follows "from this, that, even in France, no previous effects had been "produced by the labours of BOYLE, of NEWTON, and of the "other English experimentalists, trained in BACON's school." Mr STEWART farther observes, generally, "that the merits of "BACON failed, for *a century and a half*, to command the general admiration of Europe. Nor was BACON himself," he continues, "unapprised of the slow growth of his posthumous "fame. No writer seems ever to have felt more deeply, that "he properly belonged to a later, and more enlightened age; "a sentiment which he has pathetically expressed in that "clause of his testament, where he 'bequeaths his name to "'posterity after some generations shall be past *.'"

In making these statements, Mr STEWART seems to have overlooked a crowd of testimonies, which prove in the most satisfactory manner, that BACON's philosophical fame was *early* established, not only in France, but in all the other countries of Europe, where letters were cultivated. I must farther be permitted to express some doubt, whether Mr STEWART has rightly interpreted that truly affecting clause of BACON's Testament to which he so eloquently alludes. There are no contemporary publications which give any countenance to the supposition, that BACON

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* *Dissertation on the Progress of Metaphysical, Ethical, and Political Philosophy*, p. 58, 85.; prefixed to the *Supplement to the Encyclopædia Britannica*.—These statements have been already questioned, in part, in the article of the *Edinburgh Review* before referred to. The able author of that article contends, that BACON's *fame* was early and generally established throughout the Continent; but he admits, that it was late before any beneficial effects were produced by his *philosophy*.

CON himself thought his writings had not met with due attention from the learned world. We have, indeed, his own evidence to the contrary, in regard to the most important, and, as he himself says, the most "abstruse" of them,—the *Novum Organum*. "I have received," says he, "*from many parts beyond the seas*, testimonies touching that work, much beyond "what I could have expected at the first in so abstruse an "argument *." It is probable, therefore, that the bequest of his Name to future generations, referred rather to his *public* than to his *philosophical* character. In his *act of submission* presented to the House of Peers after his disgrace, he implored them to recollect, that there are "*vitia temporis* as well as "*vitia hominis*;" and he perhaps soothed his wounded spirit with the hope, that posterity would find an excuse for his frailties, in the lax notions and practices of the age; and would look upon his fall, to use a comparison of his own, "but as a "little picture of night-work, among the fair and excellent "tables of his acts and works †." The exact terms of the clause, besides, seem to countenance the interpretation, that his hopes pointed to the greater *candour*, rather than to the greater *intelligence* of after times. "My name and memory," says he, "I leave to men's *charitable* speeches, and to foreign "nations, and the next ages ‡." But whatever opinion may be entertained upon this point, it will, I hope, appear evident

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* *Epistle* to Bishop ANDREWS, prefixed to *An Advertisement touching an Holy War*, written in 1622, and published by Dr RAWLEY in 1629, in a collection entitled, *Certain Miscellany Works of Lord BACON*, 4to.

† *Epistle* to Bishop ANDREWS, prefixed to his *Holy War*.

‡ BACON's Works, vol. iii. p. 677.

in the sequel, that BACON's works were well known, and their beneficial effects largely acknowledged, in foreign countries, long before the period pointed at in the statements of Mr STEWART.

In the first place, then, I must observe, generally, that the testimony of such of BACON's contemporaries as allude to his writings, as well as of his earlier Biographers and Editors, stands decidedly opposed to the supposition, that his fame was of slow growth upon the Continent. The information which they give upon this point, rather, indeed, supports a contrary conclusion, —that the early celebrity of his writings abroad, contributed to enhance their credit at home. Thus, OSBORN tells us, that it was the voice of foreign fame which silenced the cry of atheism, raised against them by some of the School-Divines of his own country *. Mr STEWART dates the full acknowledgment of his philosophical merits *in England* from the period of the establishment of the Royal Society †. Now, in the account of BACON's Life, published in 1657 by Dr RAWLEY, who had been for many years his domestic Chaplain, it is distinctly stated, " that his fame was greater, and sounded louder in fo-" reign parts than at home;" and it is added, " that divers of " his works had been translated more than once into other " tongues, both learned and modern, by foreign pens ‡." Dr RAWLEY had, some years before, received a strong proof of the early celebrity of his late Patron's writings abroad, in a letter from ISAAC GRUTER, which contains the following passage: " LEWIS ELZEVIR wrote me lately from Amsterdam, that " he

* OSBORN's *Miscellany of Essays, Paradoxes and Discourses,* Preface.

† *Dissertation,* p. 158.

‡ *Life,* prefixed to RAWLEY's *Resuscitatio,* first published in 1657.

"he was designed to begin shortly, an edition in *quarto*, of all "the works of Lord BACON; and he desired my advice, and "any assistance I could give him; to the end that, as far as "possible, these works might come abroad with advantage, "*which have been long received with the kindest eulogies, and* "*with the most attested applause of the learned world* *." This letter was written in 1652, only twenty-six years after BACON'S death; and the important statement which it contains, in regard to the early impression made by his writings in foreign countries, will be found fully corroborated by a more particular examination of their literary records.

With respect to France, the only *direct* authority to which Mr STEWART refers, when he states it as "an unquestionable "fact," that BACON'S writings were little known in that country till after the publication of the *Encyclopedie*, is that of MONTUCLA. After quoting a short passage to that effect from the preface to this writer's *History of Mathematics*, he farther remarks, in a Note, that "BAYLE has devoted to BACON only "twelve lines of his Dictionary †." But, surely, no weight whatever can be attached to this circumstance, when it is recollected, that BAYLE has not devoted even *one* line of that work, in the shape of a separate article, either to GALILEO or DESCARTES. I must, besides, observe, that his notice of BACON, scanty as it is, yet contains enough to show, that MONTUCLA'S observation is not well founded. The article mentions, generally, that BACON'S writings "had been favour- "ably

* TENNISON'S *Baconiana*, p. 229.—Dr WATS, in the *Dedication* prefixed to his translation of the *De Augmentis Scientiarum*, published in 1674, speaks of BACON "as an author well known in the European world."—Dr SHAW, in the *Preface* to his edition of BACON'S works, published in 1733, says, that "foreigners appear to have extolled him in a superlative manner."

† *Dissertation*, p. 58.

"ably received by the world." It states, that the Treatise *De Augmentis Scientiarum* had been reprinted at Paris in 1624; that is, the year after it was published in London; and reference is made to some high eulogiums, which had been pronounced by French writers upon that important work. It farther mentions, that a number of editions of a French translation of his moral and political pieces had been called for, within a short period after its publication; a circumstance which BAYLE casually notices in another of his works, the *Reponse aux Questions d'un Provincial* *.

That BACON's philosophical views were well known in France, before his death, is a fact, for which we have an authority the more satisfactory, that it is that of the biographer and disciple of his great French rival, in the reformation of knowledge. "While DESCARTES," says ADRIAN BAILLET, in his copious and instructive life of that philosopher, "was in Paris in 1626, "he heard the news of the death of the Lord Chancellor "BACON, which happened in April of that year. The intel-"ligence very sensibly affected those who aspired to the re-"establishment of true philosophy; *and who knew, that* BACON "*had been labouring in that great undertaking for several years* "*before his death.* The accomplishment of this heroical de-"sign," continues this devoted Cartesian, "was reserved for "a still more extraordinary genius; *but the praises which* "BACON *received* were justly due, even from those who did "not approve of his plan for the reformation of philosophy †." The

* Chap. 9.—*Troisième partie.*—BACON's *Essays*, and his *Advancement of Learning*, were translated into the French language a considerable time before his death. His *Natural History*, and *New Atlantis*, were translated into that language by PIERRE D'AMBOISE in 1631. BACON's works, says this writer, "deserve a place in all libraries, and to be ranked with the noblest literary monuments of antiquity."

† *La Vie de M. Descartes*, par BAILLET, tom. i. p. 147, 148. 4to.

The same writer admits, that BACON's example may have been of some use to his French rival; inasmuch as it was calculated to encourage him in his design, to abjure the authority of the ancients, and to re-establish the sciences upon a new foundation *. He farther observes, that DESCARTES thought BACON's method very well suited to the views of those who were willing to incur the expence and trouble of instituting experiments †. In making this observation, he refers to some remarkable passages in DESCARTES's letters to Father MERSENNE; one of which is as follows: "You formerly wrote me, that you "knew persons, who were willing to labour for the advance-"ment of the sciences, at the cost of all sorts of observations "and experiments; now, if any one who is inclined this way, "could be prevailed upon to undertake a history of the ap-"pearances of the Heavenly Bodies, to be drawn up accord-"ing to the *Verulamian method, without the admixture of hypo-"thesis;* such a work as this would prove of great utility, and "would save me a great deal of trouble in the prosecution "of my inquiries ‡."

Thus,

* *La Vie de M. Descartes*, tom. i. p. 148.—DESCARTES was about thirty years of age at BACON's death, and did not publish any of his principal works till several years after that period.

† Ibid. p. 149.

‡ *Lettres de M. Descartes*, tom. iv. p. 210. Paris edit. 1724.—It appears from the following passage in one of Sir KENELM DIGBY's letters to FERMAT, the rival of DESCARTES in mathematical science, that this eminent geometer was a great admirer of the works of BACON: "Je ne sçaurois m'empécher de vous envoyer quelques vers que le plus grand genie de nôtre Isle pour les Muses écrivit au Chancelier BACON, qui étoit son grand ami, et que vous témoignez étre fort le vôtre en le citant souvent." 13. Fev. 1658.—*Lettres* de M. FERMAT, p. 198. annexed to his *Opera Mathematica*.

Thus, it is clear, that more than a hundred years before the appearance of the *Encyclopedie*, BACON's writings had attracted so much notice in France, as to force them upon the attention of those who were but little disposed to relish their philosophy. It farther appears, that the first doubts that were entertained as to the sufficiency of the method of DESCARTES, originated among those of his countrymen who had imbibed the spirit of BACON's Logic. The doctrines of the *Novum Organum* are professedly taken as the basis of the argument, in a letter addressed to DESCARTES in 1648, by a correspondent who wishes to convince him, that in physical science, no principles ought to be admitted, but such as have been previously derived from facts *. In a piece, by a different author, written some years later, entitled *Remarques sur la Methode de* DESCARTES, BACON's method is characterised as follows: " One " sees so much judgment in the rules laid down in the *Novum* " *Organum*, for guiding the understanding in the search of " truth, that one might almost believe its author had been in- " spired. This work, indeed, has some defects, particularly in " its language, which is often scholastic and fanciful; but far " from wishing to dwell upon them, we ought to proclaim, " *that it is only since the time of* BACON, *that the human mind* " *has followed a proper plan in matters of philosophy* †." It is worthy of notice, that the author of this eulogium speaks of the Royal Society of London, then recently established, in terms of great approbation; and as being likely to realise all BACON's views for the advancement of the Sciences.

GASSENDI

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* *Lettre premiere* à M. DESCARTES, prefixed to his *Treatise on the Passions*, Paris edit. 1726.

† *Remarques sur la Methode de* DESCARTES, p. 128, 129; annexed to his *Discours de la Methode*, Paris, 1724.

Gassendi was one of the earliest disciples of Bacon in France; and he was also one of the earliest and most strenuous opponents of Descartes's Philosophy. He has characterized the principles of philosophizing, which these two reformers respectively professed, in a very clear and able manner, in the *tenth* and *eleventh* chapters of his treatise *De Logicæ origine, et varietate* *. The reformation attempted by Bacon, is there pronounced a truly great and heroical undertaking. In another work, his excellent account of the life of his celebrated friend Peiresc, there is a passage, in which Bacon is mentioned in a way particularly deserving of notice in the present discussion. "No man," says Gassendi, speaking of his friend, "made more observations, or caused more to be "made; to the end, that at last some notions of natural "things, more sound and pure than those commonly received, "might be collected; for which reason, he admired the ge-"nius, and approved the design of that great Chancellor of "England, Sir Francis Bacon †." Now, Peiresc died in 1637, only eleven years after Bacon. But this is not all. He was the first man in France, according to Bailly, who deserved the name of an astronomer ‡; and he, as well as Gassendi, who was also distinguished as an astronomer,—was a correspondent, friend, and admirer of Galileo; yet we see, that Bacon was considered by both, as the great leader of reform in Natural Philosophy.

There are many other testimonies, of a similar purport, in the writings of those who were conversant with the French experimentalists. One of these, that furnished by

 Sorbierre,

* Gassendi, *Opera*, tom. i.

† Gassendi's *Life of Peiresc*, Book vi. p. 207. of the English translation.

‡ *Histoire de l'Astronomie Moderne*, liv. iii. § 20.

SORBIERRE, in his famous *Voyage en Angleterre*, published in 1664, is probably entitled to more consideration than his own name could, of itself, attach to it; for he had acted for some time as the Secretary of one of those associations of Parisian philosophers in which the *Academy of Sciences* had its beginning *. "Ce grand homme," says he, speaking of BACON, "est sans doute celuy qui a le plus puissamment solicité les interests de la physique, et excité le monde à faire des experiences †." A similar observation is made, and in words equally strong, by the Abbé GALLOIS, in one of the numbers of the *Journal des Savans*, published in 1666; a year signalized by the establishment of the *Academy of Sciences* ‡. BACON is also represented as the father of the inductive or experimental method, by JOHN BAPTISTE DU HAMEL, the person who first held the office of Secretary to that Academy. His treatise *De Mente Humana*, published in 1672, contains several chapters of commentary upon BACON's Philosophy ‖. We are told by FONTENELLE, that DU HAMEL was censured by his contemporaries, as not being sufficiently regardful of the merits of DESCARTES §. With such views as he seems to have imbibed from the writings of BACON, he must, indeed, have been but little disposed to look up to DESCARTES as the oracle of philosophy.

It

* BIRCH's *History of the Royal Society*, vol. i. p. 27.

† SORBIERRE, *Relation d'un Voyage en Angleterre.*

‡ "On peut dire que ce grand Chancelier est un de ceux qui ont les plus contribué à l'avancement des sciences."—*Journal des Savans*, du 2. Mars, 1666.

‖ Lib. i. cap. 3. § 7.; Lib. iii. cap. 6, 7, 8, 9.

§ FONTENELLE, *Eloge de Du Hamel.*

It would be quite unnecessary to proceed any farther, in accumulating French authorities. The preceding deduction is sufficient to establish, not only that there is no foundation whatever for the statement, that BACON's writings were little known in France previous to the publication of the *Encyclopedie;* but that they had, at a much earlier period, made an impression in that country, greatly favourable to the progress of truth *. I shall, therefore, go on to inquire, though in a cursory manner, whether there are any similar proofs of equally early attention having been paid to them, by the other lettered nations of the Continent.

Turning to Italy, we shall find, that there also, BACON's philosophical works had attracted considerable notice, even before his death. It is evident from his correspondence with Father FULGENTIO, that the Venetian philosophers were extremely inquisitive about his publications †. His correspondence with Father BARANZAN proves, that the *Novum Organum* was known, and had found anxious readers, in the north of Italy, at a surpri-

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* D'ALEMBERT, in his Preliminary *Discourse*, assumes, that BACON's writings remained long unheeded, and then exerts his ingenuity to show how this result was to be expected. " La scholastique qui dominoit de son temps, ne pouvoit étre renversée que par des opinions *hardies et nouvelles ;* et il n'y a pas apparence qu'un philosophe, qui se contente de dire,—*Voilà le peu que vous avez appris, voici ce qu'il vous reste à chercher,* soit destiné à faire beaucoup de bruit parmi ses contemporains." But were not BACON's opinions sufficiently bold, new, and animating, to attract the notice of an age already disposed to innovation? Did he not proclaim in the most energetic terms, that the whole of the antient systems and methods of philosophy must be abandoned as corrupt and incapable; that the true path to science had been delineated only by him; and that countless discoveries waited to reward those who should follow that path with free minds and regulated perseverance?

——" He try'd each art, reprov'd each dull delay,
Allur'd to brighter worlds, and led the way."

† TENNISON's *Baconiana*, p. 196, 197.

singly early period. BARANZAN was a Piedmontese monk of the order of Barnabites, and officiated as a Professor of Philosophy and Mathematics, in the Colleges of his order. He had early distinguished himself as a writer on philosophical subjects, and as a discarder of the authority of ARISTOTLE. After perusing the *Novum Organum*, he appears to have begun a correspondence with BACON, one of whose letters to him is fortunately preserved in the account of BARANZAN's life in NICERON's *Memoirs* *. This letter is dated in 1622, only two years after the publication of the *Novum Organum;* and was evidently written, in answer to some queries of BARANZAN, touching its fundamental doctrines. The whole letter is on this account extremely interesting; but the following passage may be cited, as more particularly calculated to show, how much philosophy then stood in need of such a guide as BACON. "De "multitudine instantiarum, quæ homines deterrere possit, hæc "respondeo: quid opus est dissimulatione? Aut copia in"stantiarum comparanda, aut negotium deserendum. Aliæ "omnes viæ, utcunque blandiantur, imperviæ." It is worthy of notice, that BACON concludes this letter with an earnest request, that BARANZAN would employ himself in framing a description of the heavenly bodies, exactly of the kind which DESCARTES afterwards wished some competent person to undertake; as mentioned in his letter, before quoted, to Father MERSENNE. But this ingenious Italian was not permitted to profit by the exhortations of his illustrious correspondent, for he died soon after the date of this letter, at the early age of thirty-three.

There

* "Elle est trop interessante," says NICERON, who possessed the original letter, "et fait trop bien connoître la maniere de philosopher, qu'ils vouloient tous deux introduire, pour ne la point communiquer au publique."—*Memoires pour servir à l'histoire des Hommes Illustres,* tom. iii. p. 43.

There is a letter from Sir TOBY MATTHEW to BACON, which contains a curious piece of information, not hitherto particularly noticed, I believe, by any of the learned. It was written from Brussels in 1619, when Sir TOBY was on his return to Florence, where, during a former residence, he had published an Italian translation of BACON'S *Essays*. "There "was with me to-day," says he, "one Mr RICHARD WHITE, "who hath spent some time at Florence, and is now go-"ing to England. He tells me, that GALILEO *had answer-"ed your discourse concerning the flux and re-flux of the sea*; "and was sending it unto me; but that he hindered GA-"LILEO, because his answer was founded upon a false sup-"position; namely, that there was in the ocean a full sea but "once in the twenty-four hours. But now," adds Sir TOBY, "I will call upon GALILEO again *." As the discourse on the *Tides*, here alluded to, was not published till several years after BACON'S death †, it must have been sent to GALILEO in manuscript. What farther communication took place upon the subject, does not appear. There is no allusion to any of BACON'S writings, so far as I know, in the works of GALILEO; though the circumstance just mentioned, and the unquestionable notoriety of these writings in Italy, during his time, render it difficult to believe, that he had not perused them. The following passage, contained in a letter written from thence to the Earl of Devonshire, near, but before the time of BACON'S death, furnishes

* BACON'S *Works*, vol. iii. p. 562.

† It was first published, I believe, by ISAAC GRUTER in 1653, in the collection entitled *FRAN. BACONI de Verulamio Scripta in Naturali et Universali Philosophia*, 12mo, Amst. The pieces contained in this collection, were given to GRUTER by Sir WILLIAM BOSWELL, the English Resident in Holland, to whom BACON had committed them by his will.

furnishes an additional proof of that notoriety. "Lord BACON," says the writer, "*is here, more and more known, and his works* "*more and more delighted in* *."

There was an Italian philosopher of that period, whose ardent genius, the cruel torture of the rack, and twenty-seven years imprisonment, had not been able to repress; who fortunately found a friend, to publish in Germany, the works which he penned in the prisons of Naples; and who has had the honour to be placed in the same rank with BACON, by no less a judge of philosophical merit than LEIBNITZ. This was CAMPANELLA. "If," says LEIBNITZ, "we compare DESCARTES and "HOBBES, with BACON and CAMPANELLA, the former writers "seem to grovel upon the earth,—the latter to soar to the "heavens, by the vastness of their conceptions, their plans, "and their enterprises."—"After looking," says Mr STEWART, (from whose rich stores of varied erudition I have borrowed this quotation,) "into several of CAMPANELLA's works, "with some attention, I must confess, I am at a loss to con-"ceive, upon what grounds this eulogy proceeds †." But, however just Mr STEWART's surprise, LEIBNITZ was not the first who conjoined the names of BACON and CAMPANELLA. TOBIAS ADAMS, the person who performed the task of editing those works which CAMPANELLA wrote in prison, tells us, in his introduction to the *Realis Philosophia* of the latter, published at Frankfort in 1623, that CAMPANELLA, like the great VERULAM, took experience for his guide, and drew his philosophy from the book of nature ‡. The comparison here, is as unsound, as the eulogy

* See BACON's Life, prefixed to RAWLEY's *Rescuscitatio*.

† *Dissertation*, p. 39.

‡ *Realis Philosophiæ Epilogisticæ partes quatuor; hoc est, de rerum natura, hominum moribus, politica, et œconomica; cum adnot.* THOB. ADAMI.

eulogy of LEIBNITZ is excessive; but it is remarkable, as showing, that the scope and objects of BACON's Philosophy were known and approved, at this early period, in Germany. We have another illustration of the early diffusion of his views in that country, in COMMENIUS's *Synopsis Physicæ ad lumen divinum reformatæ*, published in 1643; in which work, the author speaks of the *Novum Organum* in the highest terms of praise; and warns his readers, that it was not his wish to interfere with the great plan of discovery which it proposes; but to make a trial, whether the lights of Scripture might not assist in the interpretation of nature *.

Among the German writers of the *later* half of the seventeenth century, who either professedly or incidentally treat of the history of philosophy, there are various references to be found to the writings of BACON, coupled with the strongest acknowledgments of their beneficial influence. Some of them ascribe merits to his works which have been pointedly disclaimed by the more discriminating of his English admirers. Thus MORHOF, besides the other praises which he lavishes upon him, [illegible] affirms

* "Ego quia in lumine Dei lumen videre visus sum, temperare mihi non potui, quin, advocato in auxilium Deo, novas naturalium hypotheses in novam methodum redigere, discipulisque Scholæ hujus dictare, tentarim. Non quod magni VERULAMII consilio (qui ab axiomatibus, antequam de omnibus et singulis plenæ per universam Naturam inductiones exstent, abstinendum esse censet) adversus ire vellem; sed ad capiendum interea experimentum, numnam ratione hac plus luminis, ad Naturæ arcana facilius observandum, inferri possit mentibus."—COMMENII, *Physicæ ad lumen divinum reformatæ Synopsis*, Præf.

In this work, also, CAMPANELLA is mentioned in conjunction with BACON, for reasons which render the passage deserving of notice here. "Videat autem qui volet CAMPANELLAM et VERULAMIUM (hos enim HERCULES, qui debellandis monstris expurgandisque Augiæ stabulis, feliciter admoverunt manus commonstrasse; et illis, quos Aristotelicæ vanè turgidæ Philosophiæ dementatos tenet authoritas, opposuisse, sufficiat); et quam sæpè à vero aberrent Aristotelicæ assertiones, palpare poterit."—Præf.

affirms, that his works contain the germs of many important discoveries in physics, the glory attached to which, though wholly reaped by others, was partly due to him *. His services to physics, are much more correctly indicated, by another well known German writer of that period, namely, Baron PUFFENDORF. "It was the late Chancellor BACON," says he, "who raised "the standard, and urged on the march of discovery; so that if "any considerable improvements have been made in philoso-"phy, in this age, there has been not a little owing to that "great man †."

Descending somewhat lower in point of time, though keeping still within the period of the supposed abeyance of BACON's fame on the Continent, we find BUDDEUS, a writer of unquestionable knowledge, representing him, as having completed the overthrow of the authority of ARISTOTLE, and as having not only described the true method, but powerfully accelerated the progress of scientific discovery ‡. I shall only add one authority

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* MORHOFI, *Polyhistor.* tom. ii. lib. 2. cap. 1.——MORHOF gives the following notice of a work published in Hungary in 1663, in which an attempt was made to explain the principles of BACON's philosophy. "Ex mente VERULAMII quædam in sua universali methodo instituere voluit JOHANNES BAYERUS, libro cui titulus: *Filum Labyrinthi, sive Lux mentium universalis, cognoscendis, expendendis et communicandis universis rebus accensa.* Verum obscurat potius VERULAMII sensus omnemque philosophiam, quam ut lumen aliquod accendat."—*Polyhist.* tom. i. lib. 2. cap. 7. The title of BAYER's work is, partly, that of one BACON's philosophical fragments, *(Filum Labyrinthi)*; and however imperfect his work may be as an exposition of BACON's views, it shows that his philosophical writings had early engaged the attention of the learned, even in the more obscure parts of the Continent.

† *Specimen. Controvers.* Cap. i. sect. 5. apud POPE BLOUNT—*Censura Celeb. Author,* p. 635.

‡ BUDDEI, *Compendium Historiæ Philosophiæ* p. 409, 410. Edit. 1731.

authority more, that of a celebrated Dutch writer of the same day, himself an eminent improver of science in several of its branches; and who was placed in a situation, which, in a particular manner, enabled him to collect the general sentiment of Europe, upon any point connected with the history of philosophy. I here allude to BOERHAAVE; who, in his Discourse *de comparando certo in Physicis*, delivered before the University of Leyden, when he laid down the office of Rector in 1715, pronounced an eulogium upon the merits and services of BACON, which I am happy to extract as a conclusion, ornament, and sanction, to the foregoing observations.—" Atque hujus quidem Physices fortunas laudare licet ex quo magnum VERULAMIUM summo suo bono accepit! Virum certè ad omnia, quæ scientiâ humanâ comprehendi possunt, indaganda facilè principem, et de quo dubites utrum consilio, an exemplo, major fuerit in instaurandâ deformatâ Physicâ. Absque invidiâ dixero, quidquid incrementi cepit naturalis historia ab ineunte decimo sexto seculo in hanc usque horam, omne id acceptum debemus monitis et preceptis illius viri; cujus indelibilem memoriam grata colet orbis perpetuitas. Gratari quoque oportet ævo nostro, quo exire servitio sectarum licuit, sicque ardere puram, castamque, veritatem, ut, posthabitâ figmentorum atque commentorum auctoritate, Naturam solam suas dotes revelantem audiamus."

[illegible]

[illegible]

XX. *Sketch of the Geology of the Environs of Nice.* By Thomas Allan, Esq. F. R. S. Edin.

(*Read* 16*th Feb.* 1818.)

Nice is situated on the shore of the Mediterranean, in lat. 43° 41′ 16″ N. and long. east of Greenwich 7° 16′ 37″. The county of which Nice is the capital, was comprehended in the Roman province of the Maritime Alps: it was included, while under the influence of France, in a department to which the same name was given; and now, it is restored to the sovereignty of Piémont, it may be considered as bounded on the west by the Var, which separates it from France, on the north and east by the mountains of Dauphiné and Piémont, and on the south, it is washed by the Mediterranean.

Its situation is very peculiar; although scarcely ten leagues distant from one of the highest ranges of the Alps, it has a climate to boast of, equal to that of Naples or Sicily, which may be attributed to local and very peculiar circumstances. The town and the little plain of Nice, are situated immediately under the shelter of Mount Boron, which intercepts the direct blast of the Levant winds. This hill is the commencement of a series

ries which surround it on all sides, except the south: these rise with considerable abruptness, to a height of from 500 to 1000 feet, gradually increasing till they approach Mount Cao or Calvo, by some called Mount Chauve, from the bald, herbless aspect of its summit. This mountain towers majestically over the rest, and forms an imposing feature in the group. I was informed by M. Risso, that its altitude had been ascertained to be 440 toises, about 2650 feet.

Beyond this range there is a second, which separates it from the High Alps. Its most prominent points, as seen from Nice, are the Col de Brois, the Trois Mamelons, the Col d'Autillons, the Vierge d'Utelle, &c.; and then as a third defence, the Alps encircle the whole; protecting this favoured spot, on the west, by that branch which stretches through Provence, and terminates by the mountains of Les Estrelles in the sea, at a point denominated by Saussure Cap Roux; on the north, by that elevated range, whose snowy summits present a striking contrast with the ceaseless verdure of the plain below; and on the east, by that group which forms the rugged country of the Corniche, and which, by the abrupt manner it terminates in the sea, has shut up the most natural and direct access into Italy; and unfortunately, the policy of the Sardinian Government has prevented the energy and industry of mankind from being exerted in removing the difficulties which Nature has here interposed *.

It is, however, to a very small proportion of this territory that my observations extend, having been confined to within a forenoon's

* Bonaparte saw the advantage which must accrue to the country by a thoroughfare being opened in this direction, and commanded a road to be constructed, fit for every purpose between Nice and Genoa. Already one-third of this imperial undertaking is completed, one-third of the road is marked out, and the remainder

forenoon's ride of the town of Nice, one mile and a half from which, I resided.

I may notice, however, that if the circle were narrow, the country possessed peculiar advantages, in the wonderful unevenness of a surface, which, beyond the little plain, seldom presented one hundred yards of flat ground; and while it afforded the most ample opportunities for investigating its geological structure, the pleasing and diversified aspects, which this broken ground, clothed with the orange and the olive, every where presented, stimulated as well as recompensed the labours of research.

On my arrival at Nice, I by no means anticipated much interest in my examinations, from the constant occurrence of limestone; but with the assistance of M. Risso, a gentleman who has recommended himself to the scientific world, by several very interesting works on Natural History *, who kindly pointed out such objects as had previously attracted the attention of observers, I was quickly undeceived, and found, notwithstanding the apparent uniformity of the country, that it contained much matter for the contemplation of the geologist, and afforded materials which seem to bring down the operations of Nature to a less remote period than is done by the

mainder continues only practicable to mules and pedestrians. So much were the Genoese (now also Sardinian subjects) sensible of the advantage which this road would be to them, that they offered, after the restoration, to accomplish the undertaking, on being allowed to reimburse themselves by means of a toll; but the proposal was rejected. It is curious to reflect, that this road, now so unfit for the purposes of communication, was the great Roman way into Spain, and the same by which the armies of Charles V. invaded France during his contests with Francis I.

* His last publication is entitled, *Histoire Naturelle de Crustacés des Environs de Nice.* Paris, 1816.

the conclusions drawn from the examination of any other district that I am acquainted with. The observations which have within these few years been given to the world on the Paris basin, and on the similar country of Hampshire and the Isle of Wight, are extremely interesting, and in fact opened a new source of geological research; but the phenomena, as presented in both these situations, are surrounded with so many difficulties, that as yet they can hardly be considered as grounds for the foundation of any general conclusion. In the vicinity of Nice, many similar phenomena occur; but they are accompanied with indications which render the nature of their origin more evident, and consequently very greatly enhance their value.

The Limestone of Nice consists of two kinds, which, for the sake of perspicuity, I shall call the *first* and *second;* and I fear I shall be under the necessity of being contented, though not satisfied, with these appellations. For, although the distinction be so marked in the neighbourhood of Nice, as not to admit of any hesitation, yet I found, on leaving that district, when the second began to predominate, it assumed so much the texture and appearance of the first, that I could not have known the difference. I thought I might have been able to avail myself of the terms *calcaire compacte*, *calcaire caverneuse*, or Jura-Limestone. Neither of the two first are correct, as limestones of very different formations are found equally compact, and equally remarkable from the caverns that occur in them; and with respect to the name *Jura*, as a distinctive geological term, I think it extremely unfit; because, in passing the range which bears that name, from Gex to Poligny, I noticed great variety in the composition of the limestone, of which its mountains are formed. On the south side, it very much resembled the first limestone of Nice, but contained more organic remains. Before arriving at Poligny, it bore more the aspect of the limestone

stone of Derbyshire, and was accompanied with vast quantities of chert or flint, disposed in continuous lines, nearly horizontal, and parallel with the strata. In using the terms *first* and *second*, I involve no theory, although, perhaps, I would not have been far from the truth, had I adopted the more scientific language of *Transition* and *Flœtz.*

The *first* limestone, then, occurs in beds, distinctly stratified, inclined more or less to the east or north-east, being very unfavourable to vegetation, where the situations are exposed: the hills are often so destitute of covering, that the lines of the strata may be traced from the base on one side to that on the other, as distinctly as the ridges in a fresh-ploughed field.

This limestone is sometimes of a very compact texture, with an even large conchoidal fracture, generally smooth, but occasionally approaching to splintery: it breaks in sharp-edged fragments, faintly translucent, and somewhat scopiform: its colour is a pale brown or drab, and it presents no trace of crystallisation. It is in all likelihood from this variety the name of Compact Limestone was derived. In some of the strata, however, a gradual transition from the compact to the crystalline may be traced; and as the crystallisation becomes more perfect, the colouring matter disappears, so that we have it sometimes as white as the statuary marble of Carrara. Its texture also changes as the crystallisation proceeds: it first becomes splintery, and then passes on to a rough uneven fracture, consequently differing very much from the saccharine structure of the statuary marbles. In the little hollows which occasionally occur in this variety, we find the calcareous rhomb in groups of crystals very regularly formed; and where the crystallisation appears to have been pushed a little farther, the mass has become extremely friable, with a gritty feel, and seems to be entirely formed of rhombs. These strata have sometimes,

sometimes, though very rarely, a marly ferruginous matter interposed between them, but I have not observed it of more than two or three inches in thickness; it is very much given to decompose.

Of this limestone there is another variety, of a dark-brown colour, extremely compact, with the vitreous texture of our indurated sandstone, or the *Felspath ceroïde* of the French: it is less common than the others.

In the first limestone, I have found flint, not generally, but very frequently; and where the rocks were well exposed, I have seen the nodules tracing their line along the centre of the beds, exactly in the same manner as in the white limestone of Antrim, or among the chalk cliffs of the Isle of Wight. Organic remains do not frequently occur in it: they are found, however, and in one spot in remarkable abundance. The different organised bodies I noticed, are a cornu ammonis, an echinus, a pecten, and coralloids; all but the last in single detached specimens; but the coralloids abound at a spot about 150 yards east of the Lighthouse on St Hospice. It is curious to observe the effects of the weather on the surfaces of some of the blocks of this limestone. The substance of the petrified organized bodies being harder, they have resisted the action, while the matter in which they were included has been washed away; so that in some places the corals may be detached, quite relieved from the paste which once enveloped them, and forming a net-work on the surface, not unlike what we often see in the branches of ivy on the surface of a wall.

Near this, there are two or three other strata which are crowded with the remains of corals, fragments of the shell of the echinus, and various other debris. These particular beds resemble some of the limestones of England, having the organised bodies crystallised, and inclosed in a paste of compact limestone.

This

This stone is peculiarly hard, brittle, and very sonorous: the colour is a light brown, tinged with red. Here, also, the action of the weather leaves the petrified portions projecting, and produces a very rough and arid surface. I understand this coral limestone has been described by M. St Fond as a particular formation. No doubt the beds which contain these organic bodies, differ from such as contain none. They, however, form portions of the same series of stratification, and, so far as I understand the term, are parts of the same Formation.

Having described the characters and composition of the first limestone, I shall proceed to notice the caves which so frequently occur in it. These are found, not only on the shore of the sea, but high on the summits and sides of the hills, where no running water, according to the present order of things, could ever have existed. I have also seen portions of cavities displayed in a much more instructive situation, in the line of the new Genoa road, pompously denominated *Le Chemin de Rome*. Here they have been cut through in the progress of that useful, but now abandoned undertaking, and the sections laid completely open to view. To what these cavities are to be attributed, it is very difficult to determine; but from the appearance of dislocation which is observed accompanying those last mentioned, it is probable they are indebted for their formation to some great concussion; but whether proceeding from above or below, we may perhaps be better able to hazard a suggestion in the sequel.

Connected, as I conceive, with these caverns, is the broken and fragmented state in which we very frequently find the first limestone, forming the *Brêsche en place* of Saussure, one of the most interesting circumstances which occur in the history of this substance. Upon the tops of the mountains, in loose

fragments,

fragments, and in caverns, in massive columns, we find a great deal of that stalactitic limestone known by the name of *Antique alabaster*, which is likewise found filling the rents, and forming the cement of the Brêsche en place. The mode in which this substance has been infiltrated, is finely exemplified in the sections laid open in forming the new road above alluded to, where cavities occur, in some respects very much resembling those of calcedony, as seen in the trap rocks of Feroe, though on a very different scale. The calcareous matter has been deposited gently and slowly on the sides of the hollow, here and there has fallen in stalactites from the summit, either disengaged in columns, or along the sides, and then forming horizontal platforms at the bottom, like the onyx in thousands of the pebbles which occur in the amygdaloid of Scotland. Connected with all these, however, is the vein by which the infiltration was introduced; and in this lies the distinction between those calcareous and the calcedonic geodes.

Besides this species of cement, there are others, one of which forms a very compact limestone, and often presents an even conchoidal fracture, of a pale reddish-brown colour, and frequently dendritic; this passes into a coarser variety, with somewhat of a splintery fracture, and of a dark-red colour. There are some very remarkable examples of the infiltration of this material, where the great beds of limestone are not only as it were soldered together, but where, having been broken down into the smallest fragments, they are again consolidated, and form the most compact masses. Another cement is of a pale brick-red colour, more earthy in its texture; and when it forms the base of the white variety of limestone, it has a very beautiful and singular effect. The fragments are sometimes in very large masses, and sometimes in very minute particles. Where exposed to the action of the weather, the cement is frequently washed

washed from the surface, and leaves the fragments like a heap of gravel. This broken and fragmented state of the limestone, so far as I have been able to observe, is more prevalent on the outskirts and surfaces of the hills, than in places which have been worn down by the action of rivers; and in the neighbourhood of Nice it is entirely peculiar to the first limestone.

All the cements are more or less argillaceous, and if I may except an occasional appearance of pyrites, and now and then the occurrence of shells, I did not observe any other extraneous admixture. The appearance of shells, however, in the paste which agglutinates the fragments of the first limestone, is a very curious fact. They must have existed, possibly alive, but certainly, or at least in all probability, in a recent state, when this agglutination took place, and have been drawn in along with the cement. It is a fact highly deserving attention, particularly when the shells are found at a considerable elevation. I remember being much surprised at finding some minute shells included in the brescia near my house; but upon these, as the paste was so unusually superabundant, and as the spot was not exposed in a very satisfactory manner, I did not conceive I had any sufficient ground to speculate.

I have lately had an opportunity of visiting my friend Mr Rawlinson Barclay in London. It was in company with this gentleman I made most of my excursions in the vicinity of Nice. From among the specimens he had collected after my departure, he gave me one he had detached from near the summit of Mount Cao. This specimen is not only remarkable, from the situation it was found in, but from the circumstance of shells it contains being derived from very different origins. According to Captain Brown, one has every appearance of the *Turbo Fontinalis;* the second, is only the portion of a reversed shell, which appears to be part of the *Bulla Hypnorum*, retaining its colour and lustre, in a most extraordinary degree. Both these

 are

are fresh-water shells; the third, which is of a large size when compared with the others, is of the exact form of B. Ampulla of Linnæus, or Amygdalus of Dillwyn, a marine shell. That these may have fallen accidentally into fissures, and have been there conglomerated in the process of time, is quite possible; but their occurrence at such an elevation bespeaks an order of things totally different from that which prevailed at a subsequent period. I shall only further observe, with respect to the brescia itself, that it is wonderful to see how completely and perfectly the upfilling has been accomplished. Instead of the pores being choaked, through which the fluid must have passed from the surface to the interior of the rock, the liquid seems to have been absorbed as if by a sponge, and the mass presents very often a greater degree of solidity in the interior than towards the surface.—I shall now proceed to the *Second* Limestone.

This variety rests upon the first; and in this neighbourhood is composed of strata very various in their dimensions, measuring from a few inches to two and three feet in thickness. It may even exceed this, but not frequently. In this diversity in the dimensions of its strata, it presents a strong contrast with the First Limestone, whose beds are found to be remarkably uniform, when we have favourable opportunities of seeing them displayed. It is accompanied occasionally with a poor species of clay-ironstone, and a kind of blue marly clay, a substance which is very much given to decompose. Although it rests upon the first limestone, it does not maintain a conformable position: it seems as if it had been deposited immediately before the hills had assumed their present forms, and as if by means of their abrupt elevation, to have been thrown aside previous to its perfect consolidation, into the valleys in the most complete confusion,

fusion, sometimes leaning laterally against the first limestone, sometimes presenting its edges towards it, and very often singularly contorted.

With regard to its composition, it varies considerably. In colour it passes from a bluish to a brownish grey; it is sometimes very tough under the hammer; very hard and close-grained when it breaks in scopiformly angular fragments; faintly translucent on the edges, with somewhat of a splintery fracture. Again, it occurs of a dull earthy texture, breaking into rough irregular masses, and affording a strong argillaceous smell. In the compact variety, flint very often occurs, dispersed irregularly through the mass, not preserving a line, as it does in the first limestone, nor formed in the same distinct nodules, but in masses, which appear to be chemically combined with the limestone. It occasionally contains a profusion of organic remains. This is particularly the case on the peninsula of St Hospice, where in one place the rock appears to be formed of a very minute variety of nautilus, mixed with small numelites; different ostreæ, the pecten, and gryphite, are also found in it, with fragments of the shell of the echinus. It sometimes happens that the blue marly clay which accompanies the second limestone, presents itself in very thick but always conformable beds; and as nothing grows upon it in high and exposed situations, it gives a very desolate appearance to the country, which, in some places, much resembles the mining districts of Cornwall, from the heaps of naked rubbish thrown down by decomposition. This clay does not contain many shells. In a few instances, I observed them sparingly distributed, and not in the perfect state I found them elsewhere. There were among them both bivalves and univalves; of the last, one of the Trochus genus was peculiar, from being flattened.

The

The ironstone which occurs along with this, is of no great consequence, nor perhaps very general: it is not converted to any useful purpose, so far as I could learn.

There is another substance which occurs here and there among the beds of the second limestone, analogous to the mulatto limestone of Antrim, which I believe to be a compound of carbonate of lime and granular augite, similar to the coccolit of Andrada, but much finer in the grain. In both, the colour is dark green, and when the calcareous matter is separated by means of acid, the coccolit remains in minute dark-coloured grains. In Ireland, however, it occurs in more regular strata, and of considerable extent, always under the white limestone, and with the grains of augite pretty uniformly dispersed through it. Here the beds appear to be mere adventitious deposites, maintaining no conformity with the accompanying strata, extremely irregular, and of small extent. Sometimes they are entirely composed of the coccolit, when they are of a very dark green colour, and very friable: in this state, the substance is used in the manufacture of Pigments. It is generally, however, dispersed through the limestone, in which state it forms a very beautiful rock. Here, as well as in Ireland, it usually contains organic remains; besides some bivalves, the belemnite, the cornu ammonis, and the nautilus, are the shells which I have generally seen in it. It assumes positions high and low, and may be considered an independent member of the Second Limestone Formation.

There is another variety of limestone, evidently of a subsequent date, which also occurs in this vicinity, but on so small a scale, that it is scarcely deserving observation. In one place only I noticed it on the road leading from Nice to Ville Franche, where it lay in thick beds of a soft earthy texture, and containing a quantity of vegetable remains. It is of a greenish-grey colour, and probably a species of calcareous tuffa, which,

which, in a country so entirely composed of limestone, is a very common production.

Some deposites of gypsum likewise present themselves: it is of the amorphous foliated kind, and, like that of Compostello, a good deal stained with hematitic iron, but containing no crystals of quartz. The most considerable bed is one in the immediate neighbourhood of Nice: it rests on the southern extremity of the hill of Cimiez, and to the north, it abuts on the first limestone. Over it there is little else but vegetable mould. I however noticed some traces of the mulatto stone, which would induce one to consider it as a member of the second limestone. I cannot, however, determine the point, as I nowhere saw it laid open sufficiently.

Such are the materials, not alluvial, of which this interesting country is composed. A good deal of the limestone which I remarked in the south of France, between Aix and the Var, appeared to me, in passing rapidly over it, to be of the same kind as that which I have here denominated the *first*, and which I was induced to consider as belonging to the Transition series. On this point, however, I speak with diffidence, as I have had but very little opportunity of remarking its relations in combination with any apparently older or contemporaneous rock. In quitting the country by way of the Col de Tende, very shortly after leaving Nice, I did not perceive, excepting in two mountains, and there only from a distance, any traces of my first limestone: the whole country was taken possession of by the second, which imperceptibly changed its internal characters so much, that at Tende, I was for a time induced to consider it as of the older kind: it always, however, presented a striking difference in its position, being invariably highly inclined, and in many places contorted in a very remarkable degree;—nothing short, in that respect, to the most eccentric convolutions of the transition rocks of St Abb's Head.

In

In leaving the village of Tende, to ascend the tremendous pass of the Col di Cornio, by far the worst of any by which Italy is entered, the long desired change of rocks presented itself, with the limestone of Tende resting in unconformable position, and so distinct, that there was little possibility of deception. I traced this limestone to the summit of the mountain, where it rested directly on rocks which appeared to me to belong to the transition series;—from which, if my conjecture be correct, it is fair to infer, that the first limestone of Nice, is of the kind I was originally induced to suspect it belonged. It is very remarkable, however, that in England there is no similar limestone that I am aware of; nor do I know any in Scotland or Ireland which can be classed with it, notwithstanding the great extent which it occupies in this quarter of the globe.

These few substances comprehend the whole of the rocks, so far as I had an opportunity of observing, that are to be found in the vicinity of Nice; but before quitting them, I must again revert to the First Limestone, in order to describe the fissures filled with marine shells, which prevailed not only in the rock of Nice Castle, but in the adjacent promontory of Mount Boron. I name these localities, as it was there only I had an opportunity of examining them, though I have no doubt phenomena of the same kind abound throughout the neighbourhood. The fissures I now talk of, seem to have been formed after the consolidation of the brescia, already described, and are literally filled, in some places, with sea shells, of species all now alive in the Mediterranean,—a circumstance which suggested to M. Risso the appropriate name of *Marbre Mediterranéene.* Many of these shells retain their pearly lustre and colour; and although combined in masses, perfectly solid, they preserve their contour and aspect almost as fresh as the day they were deposited. The fissures which contain these organic remains, are sometimes partly filled up with solid limestone, without any appearance of petrifactions,

petrifactions, shewing that the deposition of shells had ceased and commenced again.

The occurrence of these marine remains proves, in the most satisfactory manner, that the sea must have flowed greatly above the level of these rocks, at a time when they were torn asunder in all directions; and, as if to prove that these operations had been carried on slowly, I found on the sides of some of the fissures, since filled up with the Mediterranean marble, the perforations of the pholas, with the shell of that animal remaining in its place.

But besides the hills composed of solid materials, there are others of much importance, whether considered as to their extent, their altitude, or the geological inferences which may be drawn from their structure and appearance,—I mean those formed of gravel, principally occupying the west side of the district. When travelling along the banks of the Rhone, from Lyons to Avignon, I was much struck with the enormous quantity of debris spread over a great proportion of that line, and all or nearly all belonging to the alpine range through which this mighty river flows. The debris on the banks of the Var is of the same nature, composed of masses of granite, mica-slate, quartz and jasper, pieces of compact actynolite, and serpentine; but, so far as I perceived, there were no fragments that could be referred to the trap rocks. Saussure, § 1428. states, that he saw nothing among the gravel of the Var but limestone and sandstone; but he had certainly not examined with his usual acuteness. The hills formed of this gravel rise with a sharp acclivity from the plain and the beach, forming a round-backed range, and of an uniform elevation, but rising gradually from the height of 500 or 600 feet, till they rest high on the sides of Mount Cao. Looking down upon this mass of alluvial matter from the sum-

 mit

mit of that mountain, it has exactly the appearance of having once formed a continuous bank, through which the torrents have cut their way, and produced the numerous beautiful and picturesque valleys which intersect it in all directions. Of these, the Var itself may be considered as one; and near its banks, I have seen cliffs cut in this gravel of at least 400 feet high, and quite perpendicular. The operations of the various little streams which occupy the water-courses in wet weather,—for in dry weather there is little or no water to be seen in them,—are very remarkable: sometimes I have followed them up, having barely room to squeeze myself through between the perpendicular walls, and found the cut suddenly terminate in a circular aperture, like a deep well, into which a little stream precipitated itself. In the Vallon Obscur, the opening is in some places not more than three to four feet, and the walls rise to at least 100 feet on each side, fringed at top with shrubs and trees, often hanging in frightful suspence overhead. Although there be no regular beds, the general inclination of the gravel towards the sea, is sufficiently distinct in every cut which runs in that direction; while in those that are at right angles to it, the gravel seems to be more horizontally disposed, shewing, that by some uniform impulse, it had been carried forward in one constant direction, in the same way as the Var, and other mountain torrents, transport their contributions, and deposite them in the ocean.

Unpromising as those heaps of gravel were at first appearance, they afforded another very important link in the chain of facts which this country presents. In the different hills, the pebbles are bedded in sand, which is more or less abundant. Deposites of clay are also common; sometimes they occur only in small seams, sometimes in thick extensive beds, of at least 30 to 40 feet. The colour is either of a yellowish drab, or

or a pale bluish-grey; both are mixtures of argillaceous and calcareous matter. The former often occurs without the latter, and when together, it always assumes the uppermost situation. Of the blue clay bricks and tiles are manufactured, and I think it might be applied with success to the fabrication of pottery, as it forms a tough adhesive paste when kneaded with water, and is of a delicate pale-red colour when burnt.

It is in this particular kind of clay that a considerable variety of shells are found, of kinds also which are all to be met with alive in the Mediterranean. A few of them I found in the most perfect state of preservation; but in general they were so much decomposed, that it was very difficult to withdraw them from the clay. In almost every valley, these deposites are to be found, and although some kinds of shells may prevail more in one than in another, still they are throughout generally the same varieties, and possess a character peculiar to the clay in which they occur.

In one situation, where the deposite was simply of sand, and not many inches thick, I found a great quantity of bivalve shells, principally pectens, and other small kinds of the ostrea, but not one univalve. In the particular situation I now allude to, I have no doubt that the shells were in a very recent state, if not alive, when they were left by the sea. It is true, that for one unbroken which I found, I threw aside the fragments of hundreds; yet it seems to be impossible, that shells so delicate and so minute, could submit to the action of transportation. Besides, the quantity that are here found together, bespeaks the probability of an original bed. Oyster-shells I have observed in the same bank, in so perfect a state, that I could not have believed them to be any thing but recent, had I not disengaged them with my own hands. Some of them were attached to pieces of gravel, not agglutinated by means of cal-

careous cement, but fastened, as they are in the ocean, to the soil on which they grow.

From these facts, it seems to be quite evident, that these shells must have been deposited at different periods, during the tardy formation of the gravel heaps in which they are imbedded; and also, that the heaps must have required a long time for their accumulation, not only from the growth of the beds of shells, but from the nature of the materials of which the hills are composed, and the distance from which these have been transported. When we consider the time required for the destruction of the original rocks, and for reducing them to their present state, it carries the mind back to a period, compared to which that of history is but a trifle. In the whole of these operations, we see no marks of hurry and confusion, no trace of those convulsions which must have preceded them, previous to the formation of the Mediterranean marble. On the contrary, from the disposition of the materials themselves, and from the extreme delicacy, and state of perfection in which many of the shells have been found, there is next to a positive proof, that the accumulation must have proceeded with the utmost tranquillity, and that these heaps of gravel must have been carried gradually down, and quietly deposited in the sea.

The shape which they present is also analogous to the nature of the beach in the vicinity of Nice, which, though composed of loose pebbles, is remarkably shelving, and the sea becomes suddenly very deep. SAUSSURE states, that only a league from the shore, he found 1800 feet of water. Hence, supposing a further depression of the Mediterranean, we should just have such another series of steep acclivities as the gravel banks now present, the sides and extent of which would in time be furrowed and indented by the running of torrents.

There

There are other beds containing organic remains which I consider of subsequent formation to the gravel heaps. The first of these is on the west side of the Nice plain, and about a mile from the sea; it is a bank entirely composed of sand and sea shells. They are principally bivalves, but a few minute univalves are found among them. Although the species may be considered the same, the state in which they occur is quite different from those of the blue clay, and if we except some large oyster shells, and a few pectens, the rest are in such a state of decomposition, that it is impossible to disengage them from the sand.

The second differs from this, but only in respect to the variety of shells it contains; for it is composed of the same kind of sand. It is situated on the banks of the Paglion, just above the village of Trinité. Here the sand extends over the surface of a hill, which has been brought into cultivation, and the variety of shells contained in it, is immense. When these are turned up by the *pioche* of the husbandman, and allowed gradually to lose their moisture, they become hard, and are often found in a good state; in digging for them, however, they are generally so soft, that it is very difficult to preserve them. If they can be got out entire, they afterwards attain a slight degree of hardness; but even in their pulverulent state, they exhibit their varieties distinctly, and all, I am told, are of kinds now living in the Mediterranean. Indeed, I have seen most, if not all of them, in a recent state.

These two beds appear to form a third epoch in the operations of the sea upon the surface of the land; but there is a fourth, which has all the characters of being still more recent. This presents itself on the Peninsula of St Hospice, and perhaps may involve a question with respect to the point at which shells are to be considered fossil; many of those which I have found in this situation, being in such a perfect state, that it is

nearly

nearly impossible to distinguish them from the uninjured dead shells, which are thrown upon the beach. Some of them have preserved their colour, particularly the red ones, so as to deceive any one.

St Hospice is a long narrow peninsula, which from its singularly fantastic shape, forms a most beautiful feature in the country, and is, for the most part, covered with olive trees; some of these are so large, and present such an aspect of antiquity, that they are considered by the country people to be six or seven hundred years old. The neck by which it is joined to the mainland is the lowest part of it, and there it may be seventy or eighty feet above the level of the sea. M. Risso has described* a deposite of sea-sand and shells, which was found in excavating a well in this peninsula, at the height of 20 metres above the sea-mark. They had only gone down three metres when the deposite in question was penetrated, which was found to be five metres in thickness. The shells were here discovered in such a perfect state, that when Risso presented them as fossil productions in Paris, his veracity was somewhat questioned. On the east side of the Peninsula of St Hospice, not three hundred yards from this spot, on the edge of the cliff, I found what appears to be a continuation of the above deposite but here not more than fifteen or twenty feet above the level of the sea. They lie on a mass of blue clay belonging to the second limestone, and are either imbedded in a fine white dry sand, or mixed with a proportion of clayey marl, and in one place were so abundant, that they may be taken out by the handful. The bed which contains these shells differs in thickness; in some places it may measure from 12 to 15 feet. The upper part of it is frequently so indurated, that it requires the hammer

* Journal des Mines, No. 200.

hammer to break it; elsewhere it may be dug into with a trowel. If carefully looked for, I believe it would be found to extend over a considerable portion of the peninsula, as, in walking along the edges of it, I have remarked traces of the same kind of sand, which I have no doubt are continuous.

I look upon this as the last operation of the sea previous to its being reduced to its present level. Besides the shells and fragments of coral this bed contains, I found parts of the limbs of crustaceous animals.

The last fact I have to notice, is not the least interesting, it is one which has puzzled the ingenuity of all who have attempted to account for it, and which still requires more observation in order to be satisfactorily resolved. This country, as well as many others washed by the Mediterranean, contains different deposites of the bones of animals: these are usually imbedded in a red indurated clay, forming a mass which is distinguished by the name of the *brêsche osseuse.*

This conglomerate is already so well known, that it is only necessary for me to notice the peculiarities which occurred to myself. In this neighbourhood there are three different deposites of it, one at Cimiez, another at Ville Franche, and the third on the Castle Rock of Nice. The first I have never seen. I was informed that it had recently been covered over with rubbish; of the three it is considerably the most distant from the sea. That near Ville Franche is of very small extent; the paste is of a lively brick-red colour, and possessed of a great degree of induration. The fragments of bones and teeth imbedded in it are extremely white, and along with them are some rounded pebbles of limestone, and occasionally marine shells. In one part of the deposite, where there are no bones, or, if any, in very minute fragments, the conglomerate appears to be a congeries of sea shells, mixed with the spines of the Echinus.

At

At the Castle Rock of Nice, the bones occur in two distinct states; one forming a very hard indurated brescia, the paste of which varies from a brown to a colour almost black; in the other they are loose, or feebly agglutinated, by means of calcareous infiltration, with fragments of limestone and sea-shells, showing that the sea still continued its operations when the revolution which occasioned this deposite took place.

That the sea had retired and again risen, or that a wave had flowed over this quarter, and in retiring had drawn these debris along with it, is most probable, as there appear to be several fissures very different from those formerly mentioned. Some containing a few dispersed fragments of bones, and others nothing but loose earth and stones, and which probably had been produced by some convulsion long after the formation of the Mediterranean marble; but at a period previous to, or perhaps contemporaneous with, the revolution which brought together the remains of so many different animals.

On referring to Colonel Imrie's paper, in the 4th volume of the *Transactions* of this *Society*, it appears that the bone brescias of Nice are in their construction quite analogous to those of Gibraltar, excepting that they are accompanied with traces of marine animals;—while nothing but terrestrial testacea are noticed as accompanying the other. This forms a striking distinction between them; but it is one which tends rather to involve than to elucidate the history of their formation.

Some individuals have been anxious to bring down the operations of nature in the formation of the rocks in this vicinity, even to a period subsequent to the civilization of mankind; and Saussure mentions a story related to him by the French Consul in 1787, respecting a copper nail which was said to have been found in the heart of a solid mass of limestone. Something

thing of the same kind was communicated to me by my friend Risso, who showed me three or four nails, one of which he most obligingly presented to me: these he assured me, with every appearance of conviction, were taken out of the solid strata in the vicinity of the harbour. We were very often engaged to visit the spot, but it so happened, that something always came in the way to prevent it, and I left Nice without being persuaded that the solid stratum was more than the agglutinated shingle of the sea-beach, though the nail will I believe be allowed to have all the characters of genuine antiquity.

I have thus enumerated all the objects which I met with deserving of attention in this interesting district; and although my observations were extremely circumscribed, still the materials which this little spot contains, are of much importance in the geological history of the globe. It may be remarked, however, that the phenomena which it presents are somewhat peculiar. In most countries, the organic remains are of a nature totally differing from the living animals which now inhabit them, proving that great alterations must have taken place on the physical functions, if I may use such an expression, of the different countries they are found in. In Britain, the skeleton of the Alligator, the shell of the Tortoise, and the impressions of the Palm-tree, with which its strata abound; as well as the teeth of the Elephant, and the bones of the Mastodonton, which have been found in its alluvial deposites, all tend to shew under what different circumstances that country must formerly have existed. The marine remains are in the same predicament: they are rarely, if ever, of the species which occur alive upon its coasts. At no greater distance than the opposite side of the range of hills which separate the Nice district from the

the Valley of the Po, we find a marked distinction. Besides the remains of the animals just mentioned, with those of the Hippopotamus, the Urus, and the Elk, Brocchi has enumerated 284 different shells, many of which are either the inhabitants of distant oceans, or whose prototypes are altogether unknown; while all the marine fossil remains of Nice may be found in a recent state in the adjoining sea.

Brocchi, I find, is inclined to dispute the fact of the shells of St Hospice being really fossil, as stated by M. Risso, in his short but interesting memoir on that peninsula, and is inclined to consider this spot as the bottom of some former sea. No one, I think, will dispute the last position; but with respect to the shells being really *fossil*, in a geological point of view, that must, as I have already stated, depend upon the acceptation of the term: for, here we trace these marine remains, no doubt deposited at very different epochs, from a state in which they have undergone very little alteration, to that of being reduced to an impalpable powder; and, finally, we find them imbedded in the solid rock, and even in this situation retaining a great degree of colour, and the pearly lustre among those to which it is peculiar.

In the bone brescia, we certainly find the remains of land animals no longer existing in Europe: still the state of the marine organic remains shews, that the changes which have so strangely affected other countries, do not appear to have operated on this; and they likewise prove, in the most incontestible manner, the great alteration which has taken place on the relative position of the sea and land. How this alteration has been brought about, I shall not pretend to discuss: it is an investigation somewhat analogous to that of the original formation of the Globe, or, as it is more modestly termed, the Crust of the Earth, not very likely to be rewarded by any satis-

factory

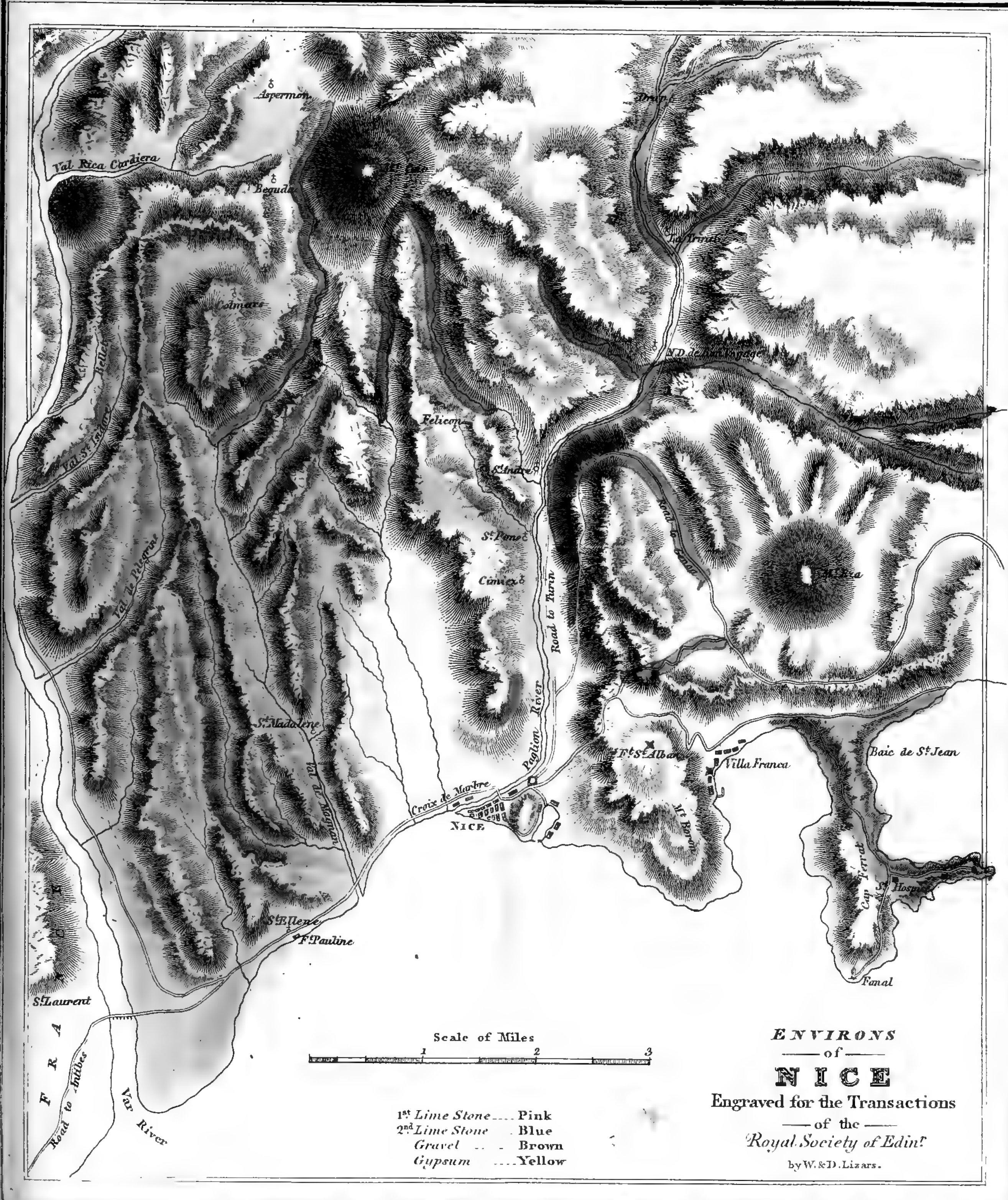
factory result. I may simply notice, however, that while BROCCHI contends that the waters of the Mediterranean have been allowed to escape by the opening of the Strait of Gibraltar, he combats the hypothesis entertained by RISSO, founded on the traditions recorded by STRABO, that the influx of the Euxine, when it first broke its way into the Mediterranean through the Thracian Bosphorus, elevated the level of the Mediterranean, while it was yet separated from the ocean. That such an event might have caused an immense wave to have inundated the adjoining coasts, is most probable; but its influence would be very transient; for all the water that could have been drained off from the Caspian and the Euxine united, by the occurrence of such a catastrophe, unless we are to suppose them to have been far more extensive than they are at present, would have very little influence on a space of such immensity as the Mediterranean.

One thing, however, appears very clear, that the alteration of the relative levels has not been occasioned by the elevation of the land, subsequent to the deposition of the gravel beds, whatever may have happened previous to that period. The characters of these alluvial heaps, on which I found my arguments, are entirely distinct from those of the transition and flœtz rocks, whose territories are so often invaded by the granite and trap formations, and elevated with every appearance of violence and agitation. The beds of gravel, on the contrary, have undergone no alteration, excepting where furrowed by the action of surface-water, since their original deposition. They appear to have been left exactly in their present situation, and exactly in the same way as the debris brought down by the continuation of the same process still continues to be, and bear the most satisfactory marks of having remained ever since in a state of perfect quiescence.

As the formation of these gravel heaps still continues to proceed before our eyes, it would form a very curious subject of speculation, if the progress of the mounds which are evidently projecting by every river which pours itself into the Mediterranean, could be ascertained. It would be curious to learn what period of years was required to make any sensible difference on the soundings at the embouchure of the Var, and although much allowance would require to be made, still accurate observations of this sort would afford some data, by which the probable period requisite for the formation of the banks above might be computed.

APPEN-

Engraved for the Royal Society Tran Vol VIII. Page 452



APPENDIX.

In the preceding sketch, I have had frequent occasion to refer to the wonderful diversity of Fossil Shells which presented themselves in different quarters of this interesting country; but in order to preserve connection, I have refrained from mixing up with the text, the description of them, when describing the different localities in which they occurred; and I am the more satisfied with having done so, because, with the assistance of Captain Brown, who has already so greatly distinguished himself as a conchologist, I am enabled to present to the Society a very distinct, and, I trust, what will be found to be a very interesting account, of these fossils. And as the work is wholly his, I hope I may be allowed at least to bear testimony to the acuteness, the unwearied and anxious perseverance with which that gentleman prosecuted his investigations, on objects truly microscopic, sparing neither time nor trouble, in satisfying himself of the accuracy of his observations, or in execution of the beautiful drawings with which he has furnished me of those which he considers new, together with a very correct drawing (see Pl. X. fig. 8.) of the specimen from the summit of Mount Cao, mentioned in the text, of which I have had occasion to take so much notice. I requested him to make this drawing, in order to preserve a specimen, that I fear, has every chance of being *unique;* a merit which, however gratifying to the self-love of the mere collector, does not add to its value in a geological cabinet. A faithful and accurate engraving is the only means of remedying this evil.

All

All the shells described in the following catalogue, are similar to those, though not in so high a state of preservation, which were found so abundantly some years ago at Grignon in France, and which abound in various parts of that country, as well as in Italy. For the most part, they are exceedingly minute, many of them requiring the power of a solar microscope to determine their characters with precision.

The whole number enumerated amounts to two hundred and twenty-five; of these, thirty-three species, which Captain Brown considers new, he has particularly described and figured. He has also described two new madrepores. Among them are some odd valves of a new *Chiton*, a genus not even noticed by Brocchi, in his *Conchologia Fossile Subapennina*: likewise several of the *Haliotis striata;* a genus also unknown, or at least not included in the work above quoted; so that all the Linnean Genera have been found in this neighbourhood except the *Argonauta.*

For the sake of brevity, Captain Brown has confined his references to the latest authority for description, and the best for the figure of the shell.

Together with the engravings of these fossil remains, I have given a small Map of the Environs of Nice. This is principally taken from a manuscript map in the possession of Mr Risso, in some degree corrected and verified by the rough observations I was able to make with one of Schmalcalder's military compasses, an instrument of sufficient accuracy for the purposes I required. It will be satisfactory to the reader to observe the bearings of the different localities; and the colouring of the map will enable him to distinguish the extent of the different formations and deposites I have had occasion to describe.

I.

I.

Fossil Shells found in the Sand and Clay, among the Gravel Heaps formed by the detritus of the Primitive Rocks of the Alps which front the left bank of the Var, and extend along the west and north sides of the Plain of Nice, coloured brown in the annexed Map.

MULTIVALVE.

LEPAS.
1. Balanoides. Dillwyn's Conch. p. 16.—Donovan, Br. Sh. I. t. 36. f. 2.

BIVALVE.

MYA.
1. inæquivalvis. Montagu, Test. Br. p. 38. t. 26. f. 7.—Dillwyn's Conch. p. 55.

TELLINA.
1. divaricata. Wood's Conch. p. 195. t. 46. f. 6.—Dillwyn's Conch. p. 102.
2. gibba. Brocchi, p. 516.—Encyc. Meth. t. 230. f. 4.

CHAMA.
1. coralliophaga? Brocchi, p. 525. t. 13. f. 10.

ARCA.
1. granosa. Linné, Syst. Nat. p. 1142.—Lister's Conch. t. 241. f. 78. and t. 242. f. 79.—Dillwyn's Conch. p. 233.
2. didyma. Brocchi, p. 479. t. 11. f. 2.

OSTREA.
1. strigilata. Brocchi, p. 571. t. 14. f. 15.
2. opercularis. Linné, Syst. Nat. p. 1147.—Donovan, Br. Sh. i. t. 12.; O. subrufa.—Dillwyn's Conch. p. 266.
3. Jacobea. Linné, Syst. Nat. 1144.—Donovan, Br. Sh. iv. t. 137.—Dillwyn's Conch. p. 248.; *Testa junior.*
4. edulis. Linné, Syst. Nat. 1184.—Pennant, Br. Zool. iv. p. 104. t. 62. f. 70.—Dillwyn's Conch. p. 280.
5. pleuronectus. Linné, Syst. Nat. p. 1145.—Rumphius, t. 45. f. A. & B.—Dillwyn's Conch. p. 250.
6. Lima. Encyc. Method. t. 206. f. 4. Lima squamosa.—Dillwyn's Conch. p. 271.

ANOMIA.
1. Squamula. Encyc. Method. t. 171. f. 6. and 7.—Maton and Racket, in Linn. Tr. viii. p. 103.—Montagu's Test. Br. p. 156. & 561.

UNIVALVE.

NAUTILUS.
1. Beccarii. Montagu, Test. Br. p. 186.; *et sup.* p. 74. t. 18. f. 4.—Dillwyn, p. 342.

BULLA.
1. cylindricea. Donovan, Br. Sh. iv. tab. 120. f. 2.—Dillwyn's Conch. p. 496.
2. ampulla. Linné, Syst. Nat. p. 1183.—Lister's Conch. t. 713. f. 69. & t. 1056. f. 8.—Dillwyn's Conch. p. 479.
3. lignaria. Linné, Syst. Nat. 1184.—Lister's Conch. t. 714. f. 71.—Dillwyn's Conch. p. 480.

VOLUTA.
1. buccinea. Brocchi, p. 319. t. 4. f. 9.
2. clandestina. Brocchi, p. 642. t. 15. f. 11.

BUCCINUM.
1. musivum. Brocchi, p. 340. t. 5. f. 1.
2. serratum. Brocchi, p. 338. t. 5. f. 4.
3. corniculum. Brocchi, p. 341. t. 15. f. 15.

MUREX.
1. turricula. Montagu's Test. Br. p. 262. t. 9. f. 1.—Dillwyn's Conch. p. 744.
2. dimidiatus. Brocchi, p. 431. t. 8. f. 18.
3. thiara. Brocchi, p. 424. t. 8. f. 6.
4. pustulatus. Brocchi, p. 430. t. 9. f. 5.
5. sutura. Pl. X. f. 7. Shell oblong, with longitudinal ribs; and strong transverse and waved obsolete longitudinal striæ. The suture is broad, and striated longitudinally; with a strong double line in its centre. Pillar-

Pillar-lip smooth, and not reflected. Spire with 10 volutions; length $1\frac{1}{4}$ inch.

6. monile. Brocchi, p. 432. t. 8. f. 15.

TROCHUS.

1. elegans. Gmelin's Linné, p. 3581.—Schrœter, Einl. i. p. 682.—Zorn Naturf. vii. p. 167. t. 2. f. D. 1. & D. 2.

TURBO.

1. clathratulus. Montagu, Test. Br. p. 297.; and Supp. p. 124.—Pennant's Br. Zool. iv. p. 129. t. 81. f. 111. A. —Dillwyn's Conch. p. 854.

2. clathrus. Linné, Syst. Nat. p. 1237.—Donovan, Br. Sh. i. t. 28.—Dillwyn's Conch. p. 854.

3. elegantissimus. Dillwyn's Conch. p. 856.—Donovan, Br. Sh. v. t. 179. f. 1.

4. eonoideus. Brocchi, p. 660. t. 16. f. 2.

5. aculeatus. Pl. X. f. 9. Shell imperforate, turrited, with sub-contiguous whirls, and thick longitudinal distant ribs, crowned with inflected spines. Volutions seven; apex pointed. This shell is very like the T. clathrus, but may easily be distinguished, from the ribs being crowned with spines.

6. tenebrosus. Maton and Racket, in Linn. Trans. VIII. p. 160.—Montagu, Test. Br. p. 303. Sup. t. 20. f. 4.

HELIX.

1. subulatus. Dillwyn's Conch. p. 881.—Donovan's Br. Sh. V. t. 172.

2. Haliotoidea. Linné, Syst. Nat. p. 1250.—Lister's Conch. t. 570. f. 21.—Dillwyn's Conch. p. 973.

NERITA.

1. Canrena. Linné, Syst. Nat. p. 1251.—Lister's Conch. t. 560. f. 4.—Dillwyn's Conch. p. 975.

DENTALIUM.

1. Entalis. Linnæus, Syst. Nat. p. 1263.—Dillwyn's Conch. p. 1065.–Donovan, Br. Sh. p. 48.—Montagu, Test. Br. p. 494.

2. coarctatum. Brocchi, p. 264. t. 1. f. 4.

II.

Fossil Shells found in the Banks of Sand at La Trinité, and on the west side of the Plain of Nice.

BIVALVE.

MYA.

1. inæquivalvis. Montagu's Test. Br. p. 38. t. 26. f. 7.—Dillwyn's Conch. p. 55. —Maton and Racket in Linn. Trans. viii. p. 40. t. 2. f. 6.

2. Trinitéa. Pl. IX. f. 3. Shell ovate, depressed, umbo a little turned to one side: posterior slope somewhat truncated; with strong regular concentric ridges, which terminate with a serrated appearance on the posterior slope. Hinge with one strong elevated tooth, which fits into a socket in the opposite valve, with a lateral tooth in the anterior slope, and a groove in the posterior; length $\frac{2}{8}$ths of an inch, breadth $2\frac{1}{2}$ eighths.

3 striata. Pl. IX. f. 12. Shell elliptical, with strong transverse striæ: hinge with an oblique tooth and small socket. Posterior end somewhat rounded; anterior end more pointed: inside smooth, with strong muscular impressions. Length $1\frac{1}{2}$ eighth, breadth $2\frac{1}{2}$.

4. undulata. Pl. IX. f. 13. Shell sub-rotund, beak sharp, and much turned to one side: hinge with a deep groove, in which the cartilage is fixed: posterior slope somewhat pointed and angular. Inside with strong longitudinal obsolete striæ, which terminate about an eighth of an inch from the base: the margin is smooth, with a strong transverse groove. Outside smooth,

smooth, with wide, transverse obsolete wrinkles. Length $\frac{5}{8}$ths of an inch, breadth the same. There is a variety of this shell more oblong, measuring $\frac{7}{8}$ths in length, and $5\frac{1}{2}$ eighths in breadth.

TELLINA.

1. rotundata. Montagu, Test. Br. p. 71. t. 2. f. 3.—Dillwyn's Conch. p. 99.
2. Radula. Montagu, Test. Br. p. 68. t. 2. f. 1. & 2.—Dillwyn's Conch. p. 194. *Venus spuria.*
3. lactea. Montagu, Test. Br. p. 70. t. 2. f. 4.—Dillwyn's Conch. p. 99.
4. tigerina. Pl. IX. f. 11. Shell sub-orbicular, with the posterior slope considerably produced: umbo turned a little to one side: hinge with two central teeth, one of which is considerably larger than the other, and bifid; and with two remote lateral teeth: margin obtuse, and slightly crenulated. Exterior surface covered with longitudinal and transverse striæ, which has much the appearance of a file. Length $4\frac{1}{2}$ eighths, breadth $5\frac{1}{2}$ eighths of an inch.
5. depressa. Gmelin's Linné, p. 3238.—Donovan, Br. Sh. v. t. 163.—Maton and Racket, in Linn. Tr. viii. p. 51.—Dorset Cat. p. 30. t. 5. f. 2.–Wood's Conch. p. 171. t. 45. f. 3.

   Tellina squalada, Solanders' MSS.—Pulteney, Dorset Cat. p. 29.—Montagu, Test. Br. p. 56.—Gualtieri, t. 88. f. L. —Chemnitz, vi. t. 10. f. 96.

CARDIUM.

1. punctatum. Brocchi, p. 666. t. 16. f. 11.

MACTRA.

1. truncata. Donovan, Br. Sh. iv. t. 126.—Fleming, in Edin. Encyc. art. Conch. p. 93. pl. 205. f. 14.—Dillwyn's Conch. p. 140.

VENUS.

1. verrucosa. Donovan, Br. Sh. ii. t. 44.—Dillwyn's Conch. p. 164.
2. Paphia. Linné, Syst. Nat. p. 1129.—Lister's Conch. t. 279. f. 116.—Dillwyn's Conch. p. 159.

3. ovata. Maton and Racket, in Linn. Tr. viii. p. 85. t. 2. f. 4.—Dillwyn's Conch. p. 171.
3. gallina. Donovan, Br. Sh. ii. t. 68. V. striatula.—Dillwyn's Conch. p. 168.
5. pectunculus. Gmelin's Linné, p. 3287.–Brocchi, Fossile subappennina, p. 560. pl. 13. f. 12.—Dillwyn's Conch. p. 184.
6. incrassata. Brocchi, p. 557. pl. 14. f. 7.
7. Islandica. A variety, of an elliptic form, and more ventricose, than the common species.—Brocchi, p. 557. pl. 14. f. 5.
8. senilis. Brocchi, p. 539. t. 13. f. 13.
9. dysera. Linné, Syst. Nat. 1130.—Lister's Conch. t. 277. f. 114.—Dillwyn's Conch. p. 161.

CHAMA.

1. calyculata. Encyc. Method. t. 233. f. 6.—Dillwyn's Conch. p. 217.
2. pectunculus. Dillwyn's Conch. p. 220.—Lister's Conch. t. 347. f. 185.
3. antiquata. Linné, Syst. Nat. p. 1138.—Lister's Conch. t. 346. f. 183.—Dillwyn's Conch. p. 215.
4. gigas. Linné, Syst. Nat. p. 1137.—Lister's Conch. t. 351. f. 189. *a.* & t. 352. f. 189. *b.*—Dillwyn's Conch. p. 213.
5. Lazarus. Encyc. Method. t. 197. f. 1.—Dillwyn's Conch. p. 221.

ARCA.

1. pilosa. Maton and Racket, in Linn. Tr. viii. p. 94. t. 3. f. 4.—Dillwyn's Conch. p. 242.
2. Glycymeris. Maton and Racket in Linn. Tr. viii. p. 93. t. 3. f. 3.—Dillwyn's Conch. p. 241.
3. nucleus. Of a large size, measuring $\frac{17}{20}$ of an inch in breadth.—Donovan, Br. Sh. ii. t. 63.—Dillwyn's Conch. p. 244.
4. didyma. See p. 29.
5. nitida. Brocchi, p. 482. t. 11. f. 3.
6. granosa. Linné, Syst. Nat. p. 1142.—Lister's Conch. t. 241. f. 78.–Dillwyn's Conch. p. 233.
7. minuta. Montagu, Test. Br. p. 140.—Donovan, Br. Sh. iii. t. 78. Arca caudata.–Dillwyn's Con. p. 245.

OSTREA.

1. pleuronectus. Vid. p. 455.
2. opercularis. Vid. p. 455.
3. strigillata. Brocchi, p. 571. t. 14. f. 15.

4. Jacobea.

4. Jacobea. Vid. p. 455.
5. arcuata. Brocchi, p. 578. t. 14. f. 11.

ANOMIA.
1. Squamula. Encyc. Method. t. 171. f. 6. & 7. Maton and Racket in Linn. Tr. viii. p. 103.—Montagu's Test. Br. p. 156. & 561.

UNIVALVE.

NAUTILUS.
1. calcar. Linné, Syst. Nat. p. 1162.—Gualtieri, t. 19. f. C.—Dillwyn's Conch. p. 340.
2. Beccarii. Montagu, Test. Br. p. 186.; and Supp. p. 74. t. 18. f. 4.—Dillwyn, p. 342.
3. depressulus. Montagu, Test. Br. Supp. p. 78. t. 18. f. 9.—Dillwyn's Conch. p. 341.
4. lævigatulus. Montagu, Test. Br. Supp. p. 75. t. 18. f. 7. &. 8.—Dillwyn's Conch. p. 341.
5. Raphanus. Linné, Syst. Nat. p. 1164.—Gualtieri, t. 19. f. L, M.

CONUS.
1. Mercati. Brocchi, p. 287. t. 2. f. 6.

VOLUTA.
1. suborbiculata. Brocchi, p. 317. t. 4. f. 3.
2. buccinea. Vid. p. 455.
3. episcopalis. Dillwyn's Conch. p. 559.—Lister's Conch. 839. f. 66.

BUCCINUM.
1. prismaticum. Brocchi, p. 337. t. 5. f. 7.
2. marginatum. Brocchi, p. 332. t. 14. f. 17. *Testa junior.*
3. polygonium. Brocchi, p. 344. t. 5. f. 10.
4. duplicatum. Linné, Syst. Nat. p. 1206.—Lister's Conch. t. 837. f. 64. —Dillwyn's Conch. p. 648.
5. gibbosulum. Encyc. Method. p. 280.—Lister's Conch. t. 973. f. 28.—Dillwyn's Conch. p. 605.

STROMBUS.
1. Pes pelecani. Linné, Syst. Nat. p. 1207.—Donovan, Brit. Sh. I. pl. 4. —Dillwyn's Conch. p. 656.
2. Pugilis. Dillwyn's Conch. p. 664.—Gualtieri, t. 32. f. 13.

MUREX.
1. distortus. Brocchi, p. 399. t. 9. f. 8.
2. angulous. Brocchi, p. 411. t. 7. f. 16.
3. dimidiatus. Vid. p. 455.
4. contiguus. Brocchi, p. 433. t. 9. f. 14.
6. triplicatus. Brocchi, p. 435. t. 9. f. 20. Murex turricula.
7. granulosus. Brocchi, p. 449. t. 9. f. 18.
8. Lingua. Chemnitz, x. p. 251. t. 161. f. 1340, & 1341.—Dillwyn's Conch. p. 688.
9. pustulatus. See p. 455.

TROCHUS.
1. perspectivus. Linnæus, Syst. Nat. p. 1227. –Lister's Conch. t. 636. f. 24.–Dillwyn's Conch. p. 784.
2. zizyphinus. Linné, Syst. Nat. p. 1231.—Donovan, Br. Sh. II. t. 52.–Dillwyn's Conch. p. 799.
3. elegans. Dillwyn's Conch. p. 807.–Zorn Naturf. VII. p. 167. t. 2. f. D. 1. and D. 2.

TURBO.
1. triplicatus. Brocchi, p. 369. t. 6. f. 14.
2. vermicularis. Brocchi, p. 372. t. 6. f. 13.
3. acutangulus. Brocchi, p. 368. t. 6. f. 10.
4. clathrus. Vid. p. 456.
5. elegantissimus. Dillwyn's Conch. p. 856.—Donovan, Br. Sh. v. t. 179. f. 1.
6. rugosus. Dillwyn's Conch. p. 829.–Lister's Conch. t. 647. f. 41.

NERITA.
1. Canrena. Vid. p. 456.
2. Glaucina. Dillwyn, p. 978.—Donovan, Br. Sh. I. t. 20. f. 1.—Gualtieri, t. 67. f. & B.

HALIOTIS.
1. striata. Dillwyn's Conch. p. 1010.–Martini's Conch. I. p. 179. t. 14. f. 138.

DENTALIUM.
1. Entalis. Linnæus, Syst. Nat. p. 1263.—Dillwyn's Conch. p. 1065.—Donovan, Br. Sh. p. 48.—Montagu, Test. Br. p. 494.
2. rectum. Gualtieri, t. 10. f. H.—Gmelin's Linné, p. 3738.—Dillwyn's Con. p. 1063.
3. Elephantinum. Linné, Syst. Nat. p. 1263.—Lister's Conch. t. 547, upper fig. 1. —Dillwyn's Conch. p. 1064.

III.

III.

Fossil Testacea found on the Peninsula of St Hospice.

MULTIVALVE.

CHITON.

1. foliatus. Pl. IX. f. 1. Shell very convex, valves strongly sulcated concentrically; the anterior valve rising into a knob. The upper part of the shell is foliated longitudinally; length about six-eighths of an inch.

Only two anterior, and one posterior valve of this shell were found.

BIVALVE.

MYA.

1. elongata. Pl. IX. f. 2. Shell concentrically wrinkled, and with longitudinal divergent striæ. Hinge with a large, broad, elevated tooth in the left valve; right valve with a small tooth and socket; into which the tooth of the opposite valve fits. Umbo placed very much to one side.
2. Trinitéa. Vid. p. 456.
3. sinuosa. Pl. IX. f. 4. Shell somewhat wedge-shaped and truncated on both ends; hinge with a small tooth. Obsoletely wrinkled, with a waved margin. Length ⅝ths, breadth ⅝ths of an inch.
4. punctatus. Pl. IX. f. 5. Shell somewhat triangular, depressed, very glossy, sub-pellucid, diaphanous, and smooth; with obsolete concentric wrinkles; towards each end, it is very finely punctured, but perfectly smooth in the centre; left valve with one central and one remote tooth; right valve with two central and one remote tooth. Umbo sharp, and turned to one side. There is a slight triangular depression, commencing about the venter, and terminating at the base. Length ⅜ths of an inch, breadth ½.
5. inæquivalvis. Vid. p. 456.

TELLINA.

1. Donacina. Montagu, Test. Br. p. 58. t. 27. f. 3.—Dillwyn's Conch. p. 89.

—Brocchi, p. 512. t. 12. f. 5. T. subcarinata.

2. rotundata. Vid. p. 456.
3. densus. Pl. IX. f. 6. Shell greyish-white, sub-orbicular, smooth, gibbous, and pellucid. Hinge with two teeth, and an internal groove in which the cartilage is fixed. Length ⅝ths, breadth the same. It is a strong and rather convex shell; resembling the tellina lactea, but differing from it in being much stronger, smoother, and the umbo being nearer the centre.
4. lactea. Vid. p. 456.
5. divaricata. Vid. p. 455.
6. radiata. Wood's Conch. p. 158. t. 38. f. 2, & 3.—Dillwyn's Conch. p. 83.
7. dentata. Wood's Conch. p. 195. t. 46. f. 6. —Dillwyn's Conch. p. 102.
8. obsoleta. Pl. IX. f. 7. Shell ovate, anterior end somewhat angulated, posterior end rounded, convex, pellucid, and glossy, with transverse, sharp, minute ridges; and a bifid primary tooth in each valve. The left valve with lateral teeth. Outside wine-yellow, with numerous, divergent, obsolete rays of a cream-yellow, which become darker as they approach the base. Inside glossy, with a large spot of straw-yellow.

Mr Allan found a recent specimen of this beautiful shell in the Mediterranean, at Nice.

Pl. IX. f. 8. In the recent species, the rays are

are of a brownish lake-red at the base of the shell, and become of a straw-yellow as they ascend, and loose themselves at the umbo; which is of a pale gallstone-yellow. In the inside is a large blotch of saffron-yellow; the rays are distinctly seen through, and deepen as they approach the base.

This shell, as well as the following species, would be placed in Lamarck's genus *Lucina*.

9. tigerina. Vid. p. 456.

CARDIUM.

1. tuberculatum. Wood's Conch. p. 210. t. 50. f. 1, & 2.—Dillwyn's Conch. p. 117.
2. multicostatum. Brocchi, p. 506. t. 12. f. 2.
3. planatum. Brocchi, p. 506. t. 13. f. 1.
4. punctatum. Brocchi, p. 666. t. 16. f. 11.

MACTRA.

1. triangula. Brocchi, p. 535. t. 13. f. 7.
2. lactea. Pl. IX. f. 9. Shell oval, milk-white, smooth, glossy, and obsoletely striated. Hinge with one small tooth, and a spatuliform cavity. Length ¼th of an inch, breadth ⅝ths.

   This shell somewhat resembles the M. Boysii.
3. solida. Donovan, Br. Sh. II. t. 61.—Dillwyn's Conch. p. 140.
4. subtruncata. Maton & Racket, in Linn. Tr. VIII. p. 71. t. 1. f. 11.—Dillwyn's Conch. p. 141.
5. Boysii. Montagu, Test. Br. p. 98. t. 3. f. 7.—Dillwyn's Con. p. 143.

DONAX.

1. trunculus. Donovan, Br. Sh. I. t. 29. f. 1.—Dillwyn's Conch. p. 150.

VENUS.

1. verrucosa. Vid. p. 457.
2. compressa. Montagu, Test. Br. Supp. p. 43. t. 26. f. 1.—Dillwyn's Conch. p. 167. V. Montagui.
3. gallina. Vid. p. 457.
4. Chione. Donovan, Brit. Sh. I. t. 17.—Dillwyn's Con. p. 178.
5. exoleta. Maton and Racket, in Linn. Trans. VIII. p. 87. t. 3. f. 1.—Dillwyn's Conch. p. 195.
6. virginia. Maton and Racket, in Linn. Trans. VIII. p. 89. t. 2. f. 8.—Dillwyn's Conch. p. 207.
7. subrhomboidea. Montagu, Test. Br. Supp. p. 49. t. 28. f. 2.
8. fasciata. Donovan, Br. Sh. V. t. 170.—Dillwyn's Conch. p. 159.
9. distorta. Pl. IX. f. 10. Shell oblong, with concentric folds, and fine, regular, divergent striæ. Hinge with two teeth in each valve, and lateral grooves in the anterior slopes. The margin is irregularly wrinkled.

SPONDYLUS.

1. gaedaropus. Lister's Conch. t. 206. f. 40. to 209. f. 3.—Rumphius, t. 47. f. E.—Dillwyn's Conch. p. 209.

CHAMA.

1. calycatula. Vid. p. 457.
2. pectunculus. Vid. p. 457.
3. Lazarus. Vid. p. 457.

ARCA.

1. Noæ. Gualtieri, t. 87. f. H.—Dillwyn's Conch. p. 226.
2. barbata. Gualtieri, t. 91. f. F.—Dillwyn's Conch. p. 229.
3. lactea. Donovan, Br. Sh. IV. t. 135.—Dillwyn's Conch. p. 236.
4. nummaria. Brocchi, p. 483. t. 11. f. 8.—Lister's Conch. p. 239. f. 81.—Dillwyn's Conch. p. 243.
5. angulosa. Lister's Conch. t. 245. f. 76.—Dillwyn's Conch. p. 240.
6. Glycymeris. Vid. p. 457.
7. pilosa. Vid. p. 457.
8. nucleus. Donovan, Br. Sh. II. t. 63.—Dillwyn's Conch. p. 244.

OSTREA.

1. varia. Donovan, Br. Sh. t. 1. f. 1.—Dillwyn's Conch. p. 260.
2. Encyc. Method. t. 214. f. 1.
3. sanguinea. Encyc. Method. t. 214. f. 4.—Dillwyn's Conch. p. 269.
4. Lima. Vid. p. 455.

ANOMIA.

ANOMIA.
1. Ephippium. Donovan, Br. Sh. I. t. 26.—Dillwyn's Conch. p. 286.
2 undulata. Donovan, Br. Sh. II. t. 45. Ostrea striata.–Dillwyn's Con. p. 289.
3. Squamula. Vid. p. 457.

MYTILUS.
1. striata. Shell concentrically striated.
Only a broken valve of this shell was found, which renders it impossible to give any further characters. It, however, has every appearance of having been formed like the M. edulis.

UNIVALVE.

NAUTILUS.
1. crispus. Montagu, Test. Br. p. 187. t. 18. f. 5.—Dillwyn's Con. p. 341.
2. Balthicus. Schroeter Einleitung, I. p. 20. t. 1. f. 2.—Dillwyn's Con. p. 342.
3. depressulus. Montagu, Test. Br. p. 190. t. 18. f. 9.—Dillwyn's Con. p. 341.
4. Beccarii. Vid. p. 458.

CONUS.
1. ponderosus. Brocchi, p. 293. t. 3. f. 1.
2. deperditus, Brocchi, p. 292, t. 3. f. 1.
3. Aldoverandi. Brocchi, p. 287. t. 2. f. 5.
4. turricula. Brocchi, p. 288. t. 2. f. 7.
5. pelagicus. Brocchi, p. 289. t. 2. f. 9.
6. coronatus. Encyc. Method. T. 322. f. 2.—Dillwyn's Conch. p. 403.

CYPRÆA.
1. Europea, Montagu, Test. Br. Sup. p. 88. Donovan, Br. Shells, II. t. 43. Cypræa pediculus.–Dillwyn's Conch. p. 467.

BULLA.
1. ovulata. Brocchi, p. 377. t. 1. f. 8. *a*.
2. retusa. Montagu, Test. Br. p. 223. t. 7. f. 5.–B. truncata, Dillwyn's Conch. p. 497.

VOLUTA.
1. quadruplicata. Pl. IX. f. 14. Shell very minute, oval, smooth; columella with four plaits, aperture linear outer lip toothed; spire not visible.
2. punctata. Pl. IX. f. 15. Shell ovate, with strong longitudinal and transverse striæ, columella two-plaited, outer lip slightly denticulated, and the aperture somewhat compressed.
3. nebulosa. Pl. IX. f. 16. Shell tapering and smooth, clouded with pale chesnut; spire prominent, being in length about equal to the body; columella, with four knobs or teeth.
4. acuta. Pl. IX. f. 17. Shell transversely striated; body large, spire very acute; the four upper whorls are granulated; pillar-lip not reflected, and having one fold; outer lip very thin.
5. clandestina, Brocchi, p. 642. t. 14. f. 4.
6. Cypræola. Brocchi, p. 321. t. 4. f. 10.
7. plicatula. Brocchi, p. 318. t. 4. f. 7.
8. mitræformis. Brocchi, p. 645. t. 14. f. 13.
9. rustica. Lister's Conch. t. 824. f. 44. t. 825. f. 45. and t. 827. f. 49.—*a* and *e* Dillwyn's Conch. p. 533.
10. paupercula. Lister's Conch. t. 819. t. 35.—Dillwyn's Conch. p. 534.
11. turgidula. Brocchi, p. 319. t. 4. f. 4.
12. Schrotereri. Encyc. Method. t. 371. f. 2.—Dillwyn's Conch. p. 539.
13. Barbadensis. Encyc. Method. t. 372. f. 6.—Dillwyn's Conch. p. 541.
14. sub-globosa. Pl. X. f. 14. Body almost globular & smooth; spire with three volutions, scarcely elevated above the body; pillar-lip thickened and thrown back on the body; with one fold; aperture oval; outer lip strong; inside dentated and striated. Length about an eighth and a half of an inch.
There are varieties of this shell differing in the length of the spire.

BUCCINUM.
1. prismaticum. Vid. p. 458

2. macula.

2. macula. Maton and Racket in Linn. Trans. VIII. p. 138. t. 4. f. 4. —Dillwyn's Conch. p. 638.
3. gibbosulum. Dillwyn's Conch. p. 605.–Lister's Conch. t. 973. f. 28.
4. minimum. Montagu, Test. Br. p. 247. t. 8. f. 2.

STROMBUS.
1. tuberculatus. Dillwyn's Conch. p. 675.— Martini's Conch. IV. p. 327. t. 157. f. 1490.

MUREX.
1. scaber. Brocchi, p. 448. t. 9. f. 17.
2. echinatus. Brocchi, p. 663. t. 8. f. 3.
3. nebula. Montagu's Test. Br. p. 267. t. 15. f. 6.—Dillwyn's Conch. p. 743.
4. Tritonis. Linné Syst. Nat. p. 1222.—Lister's Conch. t. 959. f. 12.—Dillwyn's Conch. p. 727.
5. Erinaceus. Donovan, Br. Sh. I. t. 35.—Dillwyn's Conch. p. 690.
6. angulosus. Brocchi, p. 411. t. 7. f. 16.
7. gracilis. Donovan, Brit. Sh. t. 169. f. 2.— M. emarginatus.—Dillwyn's Conch. p. 742.
8. purpureus. Montagu, Test. Br. p. 260. t. 9. f. 3.—Dillwyn's Conch. p. 745.
9. linearis. Montagu, Test. Br. p. 261. t. 9. f. 4.—Dillwyn's Conch. p. 745.
10. muricatus. Montagu, Test. Br. p. 262. t. 9. f. 2.—Dillwyn's Conch. p. 746.
11. craticulatus. Brocchi, p. 663. t. 14. f. 3.— Dillwyn's Conch. p. 740.
12. triangularis. Pl. IX. f. 18. Shell tapering, with nine whorls, well defined, transversely striated; and the whorls crossed by strong varices, from the body to the apex; three rows of them are considerably higher than the others, which gives the shell a triangular form.
13. adversus. Adam's Microscope, p. 638. t. 14. f. 21.–Dillwyn's Conch. p. 759.
14. granulatus. Rumphius, t. 30. f. L.—Dillwyn's Conch. p. 756.
15. reticulatus. Gualtieri, t. 58. f. G.—Dillwyn's Conch. p. 758.
16. trunculus, Gualtieri, t. 31. C.—Dillwyn's Conch. p. 684.
17. triplicatus. Pl. IX. f. 19. Shell turrited, with five glossy whorls, the upper volution blunt at the apex, with longitudinal ribs running from the base to the tip of the spire; columella with three plaits; beak short and slightly bent.
18. dentatus. Pl. X. f. 1. Shell turrited, strong, with six whorls deeply divided by the suture, with seven strongly warted longitudinal ribs; finely striated transversely; aperture oblong, beak short; inner lip slightly reflected, with four knobs; outer lip with a row of strong teeth within.

TROCHUS.
1. Pharonius. Lister, t. 638. f. 26.—Dillwyn's Conch. p. 772.
2. corallinus. Adanson's Senegal, p. 183. t. 12. f. 4.—Dillwyn, p. 773.
3. Guineensis. Schrœter Einl. I. p. 712.—Dillwyn, p. 773.
4. Modulus. Lister, t. 653. f. 52.—Dillwyn, p. 775.
5. tumidus. Montagu, p. 280. t. 10. f. 4.— p. 777. Tro. Patholatus,
6. fasciatus. Schrœter, I. p. 747.—Dillwyn, p. 783.
7. solaris. Lister, t. 622. f. 9.—Dillwyn, p. 786.
8. crenulatus. Brocchi, p. 354. t. 4. f. 2.
9. miliaris. Brocchi, p. 153. t. 4. f. 1.
10. tessalatus. Schrœter, I. p. 693.—Dillwyn, p. 794.
11. cinereus. Dillwyn, p. 782.—Donovan, Br. Sh. v. t. 155. f. 3.
12. torosus. Pl. X. f. 6. Shell conical, umbilicated, with five abrupt volutions; body large, in proportion to the other whorls; a deep grove runs spirally from the outer lip to the apex, in the centre of the volutions; above which is a regular row of large tubercles; the whole shell is spirally striated; and above the tubercles there are regular spots of a yellowish brown. Diam. $\frac{5}{8}$ths, length the same.

TURBO.
1. acutus. Risso in Jour. des Mines, No. 200. Aout 1813. p. 6. Rissoa acuta.
2. violacea. Risso in Journal des Mines, p. 6. Rissoa violacea.
3. Rissoa. Risso in Journal des Mines, p. 7. Rissoa plicata.

4. costatus.

4. costatus. Dillwyn's Conch. p. 860.—Walker's Minute Shells, f. 47.

5. labiatus. Maton and Racket in Linn. Trans. viii. p. 180.–Montagu, p. 362. t. 11. f. 6.

6. turgida. Pl. X. f. 3. Shell turrited, with five volutions; very much inflated; separated by a deep suture, with strong longitudinal ribs; and very fine regular transverse striæ; apex very sharp.

7. discors. Pl. X. f. 5. Shell pointed, with strong longitudinal ribs and transverse striæ; inside ribbed; body whorl, about equal to the length of the spire. Length $1\frac{1}{2}$ eighth of an inch.

8. discrepans. Pl. X. f. 4. Shell pointed, strong, with five longitudinal and transverse striæ; inside ribbed; outer lip thickened; body longer than the spire. Length $1\frac{1}{2}$ eighth of an inch.

9. glaber. Pl. X. f. 2. Shell subulate, with ten glossy well defined whorls, white, aperture sub-rotund.

10. minutus. Pl. X. f. 13. Shell conic, body large and turgid; spire short, with four moderately rounded volutions; apex rather obtuse; the whole shell is covered with fine spiral striæ, and clouded with pale honey-yellow; aperture nearly round; pillar-lip slightly reflected, and forms a sub-umbilicus; outer lip thin. Length about $\frac{1}{18}$th of an inch, breadth about half its length.

This shell might be mistaken for the Turbo graphicus, Wernerian Memoirs, vol. ii. p. 521. pl. 24. f. 6.; but it differs from it in being broader in proportion to its length, in the spire having only four volutions, and in the aperture being rounder.

11. breve. Pl. X. f. 10. Shell longitudinally ribbed; strongly striated transversely between the ribs; body large and spire short, with five well defined volutions, aperture ovate, pillar-lip smooth; outer lip thin and crenated. Length $\frac{1}{8}$th, breadth not quite an eighth of an inch.

12. truncatus. Maton and Racket in Linn. Tr. p. 177.—Montagu, Test. Br. p. 300. t. 10. f. 7.

13. pullus. Dillwyn, Br. Sh. p. 822.—Donovan, Br. Sh. I. t. 2. f. 2. to 6.

13. cimex. Dillwyn, p. 821.—Da Costa's Br. Conch. p. 104. t. 8. f. 6. & 9. Turbo cancellatus, Dorset. Cat. p. 59. t. 14. f. 6. & 9.

14. phasianella. Risso in Journal des Mines, No. 200. Aout, p. 6. Phasianella rubra.

15. geniculus. Brocchi, p. 659. t. 16. f. 1.

16. elegantissimus. Vid. p. 458.

17. striatus. Brocchi, p. 383. t. 6. f. 7.

18. Ulvæ. Dillwyn's Conch. p. 840. Pennant, Br. Zool. iv. p. 132. t. 86. f. 120. Dorset Catal. p. 49. t. 18. f. 12.

19. interruptus. Dillwyn's Conch. p. 841.–Montagu, Test. Br. p. 329. t. 20. f. 3.

20. parvus. Dillwyn's Conch. p. 857.—Adams in Linn. Trans. iii. p. 66. t. 13. f. 29. & 30. Turbo æreus, Donovan, Br. Sh. t. 90. T. lacteus.

21. cancellata. Brocchi, p. 377. t. 7. f. 8.

22. pusillus. Brocchi, p. 381. t. 6. f. 5.

23. conoides. Brocchi, p. 660. t. 16. f. 2.

24. verrucosus. Pl. X. f. 12. Shell with a ventricose body; spire with four very distinct volutions; the whole shell is covered with elevated knobs in regular rows; the interstices deeply punctured; aperture ovate; outer lip strong; pillar-lip slightly reflected towards the base; inside very glossy. Length about $\frac{1}{8}$th, breadth somewhat less.

25. gracilis. Brocchi, p. 382. t. 6. f. 6.

26. striatulus. Dillwyn's Conch. p. 857.—Montagu's Test. Br. p. 306. t. 10. f. 5.

There is also a variety of this shell with strong, elevated, longitudinal varices.

27. rugosus.

27. rugosus. Dillwyn's Conch. p. 829.—Lister's Conch. t. 647. f. 41.—Gualtieri, t. 65. f. F. & H.

28. tigerina. Pl. X. f. 11. Shell with strong longitudinal and transverse striæ; the longitudinal striæ terminate where the outer lip joins the body, and give it the appearance of fine papillæ: the body is large and ventricose; spire with five short whorls abruptly tapering to a sharpened point; aperture ovate; outer lip thin and strongly crenated; pillar-lip smooth near the base, and slightly reflected, forming a sub-umbilicus. Length, an eighth and a half, breath $\frac{1}{8}$th of an inch.

29. acinus. Brocchi, p. 381. t. 6. f. 4.

HELIX.

1. vitrea. Dillwyn's Con. p. 919.—Chemnitz p. 383. t. 15. f. 15. & 16.

2. polita. Dillwyn, p. 881. Tur. polita, Linn. Trans. viii. p. 210. Donovan, Br. Sh. t. 177.

3. haliotoidea. Dillwyn, p. 973.–Lister's Conch. t. 570. f. 21.—Montagu, Test. Br. p. 211. t. 7. f. 6. and Vig. 2. f. 6. Bulla haliotoidea.

NERITA.

1. Glaucina. Dillwyn, p. 978.—Donovan, Br. Sh. I. t. 20. f. 1.—Gualtieri, t. 67. f. A. and B.

HALIOTIS.

1. striata. Vid. p. 458.

DENTALIUM.

1. Entalis. Vid. p. 458.

PATELLA.

1. Virginea. Dillwyn's Conch. p. 1052.—Linn. Trans. IV. p. 235.—Donovan, Br. Sh. I. t. 21. f. 2.

2. Græca. Linnæus, Syst. Nat. p. 1262.—Dillwyn's Conch. p. 1056.–Donovan, Br. Sh. I. t. 21. f. 3. Pat. reticulata.

3. pellucida. Linnæus, Syst. Nat. p. 1260.—Dillwyn's Conch. p. 1042.–Donovan, Br. Sh. I. t. 31. f. 1.

4. vulgata. Linnæus, Syst. Nat. p. 1258.—Dillwyn's Conch. p. 1032.–Donovan, Br. Sh. I. t. 14.

5. fissura. Linnæus, Syst. Nat. p. 1261.—Dillwyn's Conch. p. 1054.–Donovan, Br. Sh. I. t. 3. f. 2.

6. saccharina. Linnæus, Syst. Nat. p. 1258.—Dillwyn's Conch. p. 1023.

7. equestris. Dillwyn's Conch. p. 1015.—Lister's Conch. t. 546. f. 38.

SERPULA.

1. Triquetra. Linnæus, Syst. Nat. p. 1265.—Dillwyn's Conch. p. 1073.—Martini Conch. III. t. 24. f. A.—Pulteney in Huch. Dorset, p. 53. t. 22. f. 5.

2. vermicularis. Linn. Syst. Nat. p. 1267.——Dillwyn's Conch. p. 1083.—Donovan's Br. Sh. iii. t. 95.

3. anguinia. Linnæus, Syst. Nat. p. 1266.—Martini, I. t. 2. f. 13. B, C.—Dillwyn's Conch. p. 1080.

4. arenaria. Linnæus, Syst. Nat. p. 1266.—Martini, I. t. 3. f. 19. A.—Dillwyn's Conch, p. 1078.

5. lumbricalis. Linnæus, Syst. Nat. p. 1266,—Martini, I. t. 1. f. 12. B.—Dillwyn's Conch. p. 1077.

ZOOPHYTA.

MADREPORA.

1. striata. Corals nearly straight; divergent, covered, with fine longitudinal striæ.

2. pistularia. Clavate, turbinate, base, with small pustules, and undated; horizontally ridged; centre concave; gills about 36, with intermediate ones, oval. Length, five and a half eighths, breadth three eighths of an inch.

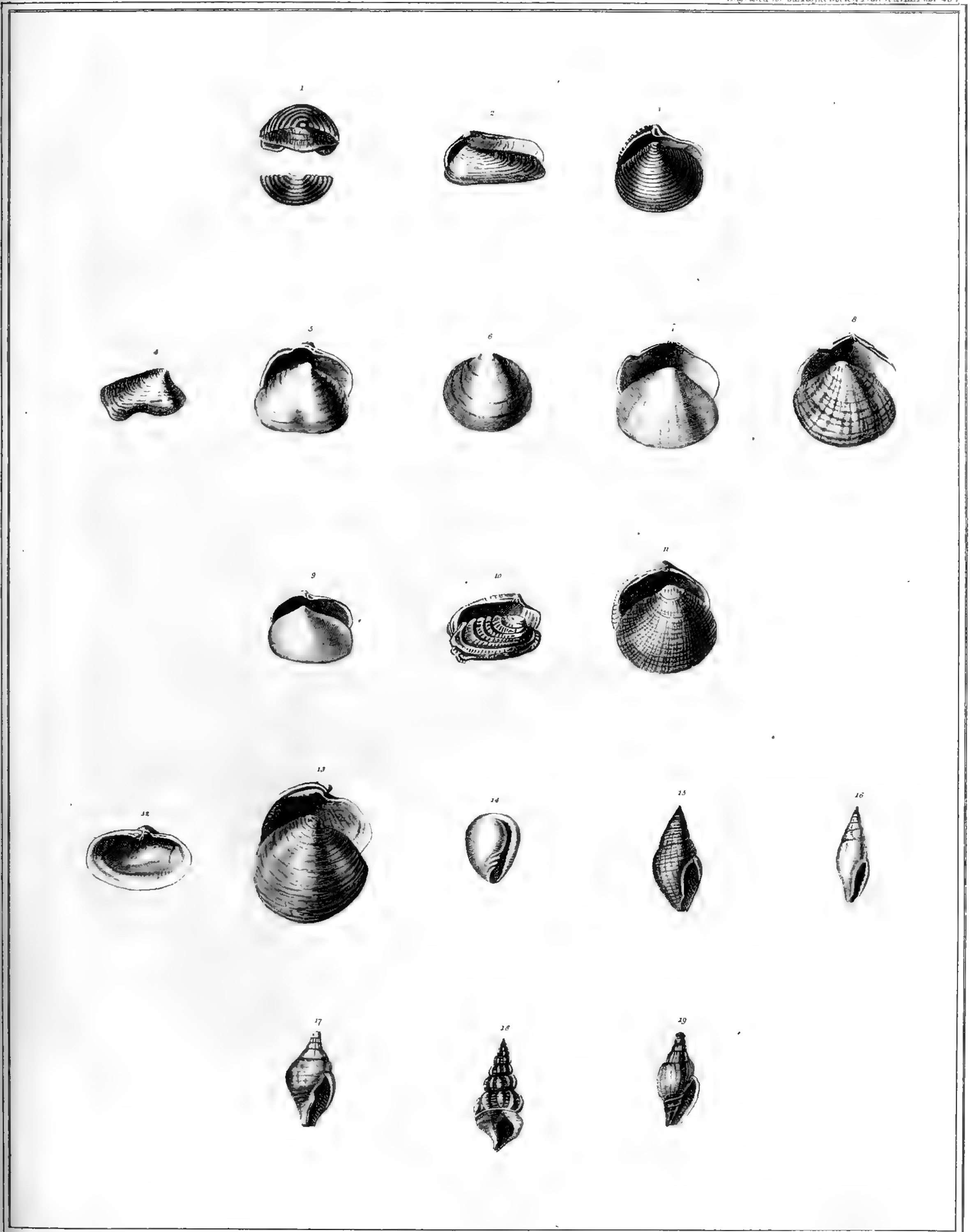
There is a variety of this with only 26 gills.

3. ramea. Turton's Linné, iv. p. 630.——Shaw, Nat. Miscell. t. 194.

4. hirtella. Turton's Linné, iv. p. 631.—Pallas El. Zooph. p. 313. No. 182.

PLATE IX.

Engraved for the Royal Society Tran Vol.VIII. Page 464



Drawn by Captⁿ Brown

Engraved by W & D Lizars Edinburgh.

PLATE X.

Drawn by Captⁿ Brown.

Engraved by W. & D. Lizars Edinburgh

XXI. *On certain Impressions of Cold transmitted from the Higher Atmosphere, with the Description of an Instrument adapted to measure them.* *By* JOHN LESLIE, F. R. S. E. *and Professor of Mathematics in the University of Edinburgh.*

(*Read March* 16. 1818.)

THE distribution of Heat over the surface of our Globe, is a capital object in the economy of Nature. The infusion of that active element communicates to bodies the principle of motion, and quickens the ceaseless revolution of the circle of generation and decay. But Heat, unlike air, water, or earth, appears never in a distinct and separate form: It exists only in a state of combination with other tangible substances; among which, it migrates from one to another. On the regulated tide of this transmission, depends the stability of the present order of things. A very small portion of the vast scale of heat is requisite and salutary for vegetable or animal life. A genial warmth fosters the powers of vegetation,—but pushed farther, it soon dries up the juices, and shrivels the leaves and tender shoots; on the other side, again, when reduced to a low temperature, it benumbs the energy of production, and finally stifles the expansion of life.

If the transfer of heat among bodies were much slower, therefore, than what actually obtains, its inequalities would accumulate, and the greater part of this fair globe would be-

 come

come a desert. The tropical countries would be burnt up by unmitigated fervour; while eternal frost, usurping the polar regions, would extend its dominion within the temperate zone. The little portion of the surface left to animation, would be blasted by the excessive inequalities of the seasons, and of the diurnal vicissitudes. The sultriest days would have invariably closed in nights of pinching cold, and this mild climate would have exhibited all the rigours of a Canadian winter, succeeded by the fervid heat of an oppressive summer.

The opposite condition leads to results still more appalling. If heat encountered no impediment in its internal motions, but diffused itself with almost instantaneous effect, it might amuse the fancy to contemplate for a moment the vast and tremendous consequences. An uniform and unvarying temperature would then pervade the globe; no distinction of climate would exist, no vicissitude of seasons, and no grateful alternation of day and night. The azure vault of heaven, pertually serene and cloudless, would lose all its animated charms. If snow and hail would be unknown, so would likewise the refreshing influence of rain and dews. The face of the earth would present a monotonous picture of sterility; no verdure to relieve the eye,—no vegetation,—and no sustenance for animals. All the springs of life would be locked up. The beneficial effects,—the very existence, of artificial heat, would for ever have been concealed; since the instant this was generated, it would spread and ingulf itself in the general mass.

It is, therefore, an important study, to discriminate the circumstances which modify and regulate the distribution of heat among bodies. Towards a correct knowledge of the subject, very considerable advances have been made, though some vague and crude notions are still suffered to prevail. To comprehend rightly the observations and reasonings contained

in

in the paper now submitted to the Society, it will be hence expedient to take a retrospect of the facts which have been disclosed relative to the communication of heat. In framing this abstract, I shall abstain from all hypotheses, which might assist, only to deceive, the imagination, but content myself with stating the facts really ascertained, and with tracing out the consequences to which they distinctly lead.

I have observed, that Heat never appears in a detached form. Yet its materiality is evinced, by the expansion which it invariably communicates to the substances with which it unites. While it is attracted by any bodies, therefore, it must introduce into them some repulsive force, which had previously existed among its own particles. Heat must hence be a fluid of extreme tenuity, yet endued with an irresistible elastic force. But when a substance, either from a chemical or mechanical impression, suffers a sudden change of constitution, and makes a copious discharge of heat, this emission is always accompanied by an effulgent light. On the other hand, when the rays of the sun, or even those of a bright lamp, are intercepted by any body, and seem lost or extinguished, a corresponding increase of warmth is immediately betrayed at the absorbing surface. It follows from this and similar facts, that heat is the same fluid as light, only in a state of rest and combination. In short, heat is latent, invisible, light,—the αφαντον φῶς of the ancients,—which has been arrested in its rapid flight, and cannot again be liberated, without such a violent change of condition, as will leave entire the repulsion of its own particles, to create the requisite projectile force. In ordinary cases, however, the light remains imprisoned in the substance with which it combines, and only migrates from one portion to another.

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The progress of heat through different bodies, depends on their peculiar constitution, and varies extremely in its rate. To examine this phenomenon more closely, it will be proper to distinguish the conducting media into the three classes of —*solid*—*liquid*—and *gaseous*.

1. In the case of a *solid* conductor, if one end of a rod be heated, the effect will gradually advance, yet not with a continuous flow, to the other end. If the rod were conceived to be distinguished into elementary spaces, each of these in the progress of communication must experience, within imperceptible limits, an alternate accession and reduction of heat, and must consequently first expand and then contract. In fact, it absolutely could not receive and deliver heat at the same instant of time, for a contemporaneous expansion and contraction—the necessary result—would imply an absurdity. But contraction naturally succeeds to expansion, as a part of the same vibratory impression. Those elementary conducting spaces, must therefore undergo an alternating hot and cold fit; so that the communication of heat through a solid substance, is performed by a series of minute articulate motions, resembling the progressive vermicular action by which the creeping insects are enabled to advance along the ground. The celerity of the transfer of heat through any solid is hence determined by the extent of such articulations, and the rapid succession of the internal oscillations; which again depends on the elasticity of the conducting substance, modified by the influence of its density.

2. In the communication of Heat through *liquids*, an auxiliary principle, originating in the internal mobility of the medium, concurs to accelerate its progress. The warmed portions of the fluid, acquiring expansion, rise upwards by their consequent

quent buoyancy, and spread over the surface. In a short while, therefore, the heat is distributed through the whole liquid mass, in successive strata, from the bottom to the top. But liquids also conduct heat by a slow process in every direction, like solids. This protracted diffusion of influence, which is exerted in restoring the equality of temperature through the body of the liquid, succeeds to the operation of the principle of buoyancy. While the surface becomes again colder, the bottom grows gradually warmer; the heat working downwards, by a continual transfer, from stratum to stratum, of the stagnant fluid.

The diffusion of heat, then, depends chiefly on the expansion and internal mobility of the liquid medium. Thus, alcohol rapidly diffuses heat, while the viscid oils, especially at low temperatures, clog its motions. The circulation is in general quicker when the liquid is very warm, its expansibility, and its aptitude for internal migrations being now increased. In approaching the boiling point, water expands largely by equal accessions of heat; but near congelation, its expansions or contractions are extremely small. The conducting power of water at a very low temperature, is hence nearly the same as if it were a solid, or remained motionless; the same, in short, as if by the addition of isinglass, it were changed into a thin jelly.

Such are the conducting powers which a liquid substance combines when at rest, or left merely to the play of its internal motions. But if made to flow in a stream, it will evidently, in consequence of the frequent renewal of contact, abstract heat more quickly from every warm solid body which is immersed in it. Thus, I find, that, in ordinary circumstances, water, advancing at the slow rate of a mile in three hours, will yet conduct away heat twice as fast as when quite stagnant. Consequently a cur-

rent

rent of three miles an hour would accelerate tenfold the transfer of heat.

It isof essential importance, however, to remark, that in all cases where the medium of communication is a liquid, the nature of the hot surface, whether vitreous or metallic, rough or polished, has no influence whatever in modifying the rate with which the heat is drawn off and dispersed. Thus, a hollow tin ball filled with hot water, will lose its heat just as fast when immersed naked in cold water, as if it were covered with linen, or paper, or a coat of any sort of pigment.

3. Heat is transferred through a *gaseous* medium by a more complex process. It is partly conducted, as in the case of liquids, by successive communication through the stagnant mass, joined to the more copious effect of the buoyant streaming of the heated portions of the fluid. But another auxiliary principle, depending merely on the nature of the heated surface, now comes into action. Let two equal hollow balls of bright thin silver, the one naked, and the other covered with a fold of cambric or paper applied closely to its surface, be filled with warm water, and suspended in a close room. The former will be found to lose 11 parts of heat, in the same time that the latter sheds 20 parts. Of the heat thus spent, 10 parts, from each of the balls, are communicated in the ordinary way, by the slow recession of the particles of air, as they come to be successively heated by their proximity. The remaining portions of heat, consisting of 1 part from the bright metallic surface, and 10 parts from the cambric, are propagated through the aërial medium, by some peculiar process. In like manner, two equal hollow balls of glass, the one gilt with gold or silver leaf, being filled with warm water, and suspended in a room, the naked one will discharge 13 parts of heat, while the gilt one will lose only 7 parts; 6 parts being spent by each, in the same way as if they had been immersed

mersed in a cold liquid, while the vitreous surface projects 7 parts, and the metallic surface only 1 part. The same differences depending on the nature of the superficial boundary, are observed to take place in the heating as well as in the cooling of bodies. Thus, if the silver balls be filled with water colder than the temperature of the room, they will acquire heat in the same proportions as they before lost it. The naked ball will gain only 11 parts of heat, while the coated will receive 20 parts.

But the *air* is still an essential vehicle of these various impressions of heat or cold. An absolute vacuum is unattainable in Nature; but the dispersive effects are always diminished, though slowly, by rarefying the medium. Thus, when the air is rarefied about 200 times, the abductive power from the glass balls will be reduced from 6 to $1\frac{1}{2}$; while the peculiar discharge of heat at the naked surface is depressed from 7 to 5, and that at the gilt surface from 1 to $\frac{3}{4}$; the naked ball now emitting $6\frac{1}{2}$ parts of heat, and the gilt one only $2\frac{1}{4}$.

The effects are changed in a different gaseous medium. Thus, the same balls, with a vitreous and a metallic surface, would discharge 31 and 25 parts of heat, if immersed in hydrogen gas; both of them now losing 24 parts by the powerful abduction of this gas. But if the medium be rarefied about 200 times, the quantities of heat emitted, from the naked and the gilt ball, will be reduced to 13 and $8\frac{3}{4}$.

As a surface of glass, or, still better, one of linen, paper, or vegetable pigment, projects heat the most copiously; so those surfaces likewise intercept the impressions most effectually. But a bright metallic surface detains only the tenth part of these impressions, and reflects all the rest. Hence the power of a metallic speculum, contrasted with that of a glass mirror, in concentrating

centrating the heat or cold projected from any remote body. Hence also the construction of the *Pyroscope*, a delicate instrument, adapted to distinguish and measure those peculiar impressions of heat or cold. It consists of a differential thermometer, having one ball naked, and the other coated with gold or silver leaf; this metallic surface reflecting the greatest part of the projections, while the vitreous surface absorbs almost the whole effect. If a broad plate of glass be interposed before a cubical canister holding hot or cold water, the action on the pyroscope, though concentrated by a reflector, will yet be very much reduced. But, on removing the plate farther from the canister, and therefore nearer to the reflector, this action will be still more diminished; so as at last to become almost extinguished. Hence the projected influence of heat or cold consists not in any streaming matter, for the same proportion of the warm or frigid rays would evidently be intercepted by the glass screen in whatever part of the route between the canister and the reflector it was planted.

But the question is completely decided, by varying the experiment. Let two glass plates be covered, each on one side with tin-foil; applying the metallic surfaces now together, place the double screen immediately before the hot canister, and the effect on the pyroscope will be greatly reduced; reverse the position of the plates, by exposing the coated sides, and the action on the sentient ball will be entirely extinguished. The interposition of a screen would in every case detain the whole impressions, if it did not itself become affected by them, and therefore come to act as a secondary but feeble projecting surface. Hence the different influence of a metallic screen, in comparison with one of glass or paper.

The impressions of heat or cold emitted from a body are not sent equally in all directions. Those projected perpendicular

cular to the surface of a cubical canister containing either warm or iced water, are the most intense, and the rest appear to diminish in the ratio of the sine of obliquity. Hence the action exerted on the sentient ball of the pyroscope, is always proportional to the visual angle subtended by the propellent surface.

Nor are those impressions necessarily propagated in straight lines. If connected rings of pasteboard be fashioned into a sort of cornucopia, its mouth being directed towards the fire, notwithstanding the twisted form of the passage, a very considerable action will be indicated on the ball of the pyroscope, presented at the narrow end.

From all these combined observations it follows, that the portion of heat or cold, of which the discharge depends on the quality of the surface, is propagated by the vehicle of its gaseous medium, though not by any actual streaming of fluid matter. No alternative then remains, but to admit that the impressions of heat or cold are conveyed through the air with a spreading and progressive tendency, in the same manner as the pulses of sound. The aerial medium, by a series of internal oscillations, successively transfers its charge, and delivers an impression at the end of the chain of communication, of the same kind precisely as it had received at the beginning.

This rapid transmission of Heat to a distance from its source, has been hastily termed *radiant*, and various inaccurate conceptions are entertained concerning its mode and extent of action. 1. In the first place, then, the process now described never obtains, unless where a *difference* of temperature occurs; and it only contributes, along with other active causes, to restore the equilibrium of heat. 2. But, in the next place, it has in every case a subordinate share only in the diffusion of heat. When the air is perfectly still, and of the ordinary density, the tide of heat vibrated from a vitreous surface amounts scarcely

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to half of the whole discharge, and from a surface of polished silver, it exceeds not the twentieth part of the entire expenditure. But such are the accelerating effects of a current of air, that, in a high wind, the pulsations of heat darted from a vitreous and a metallic surface may not form the twentieth and the two hundredth part. In a medium of hydrogen gas, those pulsations are comparatively feebler, reaching, in the case of a stagnant atmosphere, only to the eighth or the eightieth part of the full discharge, with a proportional diminution when the affected surface is exposed to the action of a current.

The influence of the pulsatory emission of heat or cold, has, therefore been greatly exaggerated. It comes merely as an auxiliary to the other modes of restoring the equilibrium of the igneous fluid, and it often contributes a very small share only towards the general effect. But whenever a body, left to itself in the atmosphere, is observed to change its temperature, some pulsatory action may be presumed to combine with the operation.

It is easy, in any case, to ascertain the *real direction* of the aërial pulses. Whether a substance acquires or loses heat, it always approaches to the temperature of the ambient medium by the very same process. The hot or cold pulses are projected *from* this approximating surface with an expansive sweep. On the contrary, when a body maintains either a higher or a lower temperature than that of the surrounding atmosphere, like the sentient ball of the pyroscope in the focus of a reflector fronting a charged canister, it *receives* and absorbs the impressions vibrated at a distance. The intervention of a paper screen does not prevent the spontaneous emission of hot or cold pulses, but will effectually obstruct their passage from a remote object.

Some theorists have imagined, that the escape of heat requires free space or vacuity. But a body will either lose or gain

gain heat with the same facility, in a small closet, as in a spacious room. Let a glass ball of 3 inches in diameter, and filled with hot water, or a frigorific mixture, be suspended in the centre of a balloon of 18 inches in diameter, and it will be found to change its temperature almost at the same rate as in a close apartment. If the balloon were much smaller, however, the effect would become sensibly diminished, because the whole of the included air, being rendered somewhat hotter or colder than before, would excite a feebler action at the surface of the ball. A similar modification would take place if the ball were formed of hollow silver. Yet this large shell of glass evidently forms a complete screen, which will absorb all the impressions emitted within it.

The sun is the great fountain of that heat which vivifies our planet. Of the solar beams, part are detained in their passage through the atmosphere, and the rest are absorbed at the surface of the earth. From experiments with the photometer, it follows, that, even when the sky is most serene, only one-half of the sun's light, sloping at an angle of 25°, will reach the ground; and that, at an angle of 15°, the proportion is reduced to one-third; but with an obliquity of 5°, only the twentieth part of the whole can gain the surface. The effect of this absorption during the day, is to raise the temperature of the external crust of the globe. Very few of the incident rays are again reflected from the surface. An expanse of sand, or even of chalk, however offensive by its white glare, yet scarcely sends back a fifth part of the light into the mass of air. A green sward, or a dark soil, absorbs almost the whole of the luminous particles.

Of the heat thus accumulating at the earth's surface, a small portion penetrates into the soil, but the greatest share is dispersed by communication to the ambient air. In the progress of the

 day,

day, therefore, the ground always becomes warmer than the incumbent stratum of atmosphere. This effect is greatest in the tropical countries, where the sun gains a higher altitude, and pours his light in a full stream. Winds very materially check the accumulation of heat at the surface, and the calorific action is besides diminished in cloudy weather. A ploughed field is more affected by the sun's rays than a grassy plot, since a loose or spongy superstratum, by exposing multiplied surfaces, dissipates more quickly the impressions of heat communicated to it.

The best mode of examining the difference between the temperature of the surface and that of the incumbent air, is by means of a pendant differential thermometer, from one to three feet in length. It consists of a ball and long stem, to which another similar ball, with a short portion of tube, having its bore swelled into a narrow cylindrical reservoir, is hermetically joined. The reservoir exceeding not the tenth of an inch in diameter, detains and supports the tinged sulphuric acid by its capillary attraction, (see fig. 5. Pl. XI.) This instrument being suspended in a vertical position, the lower ball approaching or resting on the ground, while the upper ball, at a moderate elevation, is encircled by the incumbent stratum of air, the rise of the coloured liquor in the stem will mark the excess of warmth below, and indicate very minute differences of temperature. Last summer I made some observations of that sort, but not so extensive as I have since projected. The effects were found to be extremely various. In sun-shine, and calm weather, the ground was sometimes thirty millesimal degrees warmer than the air only a few inches above it *. But when the sky happened to be much overclouded, or when strong winds swept over the surface, the

* It may be proper to repeat, that, in all the combinations of the differential thermometer, the divisions of the scale are made to correspond with the thousand parts of the interval between freezing and boiling water.

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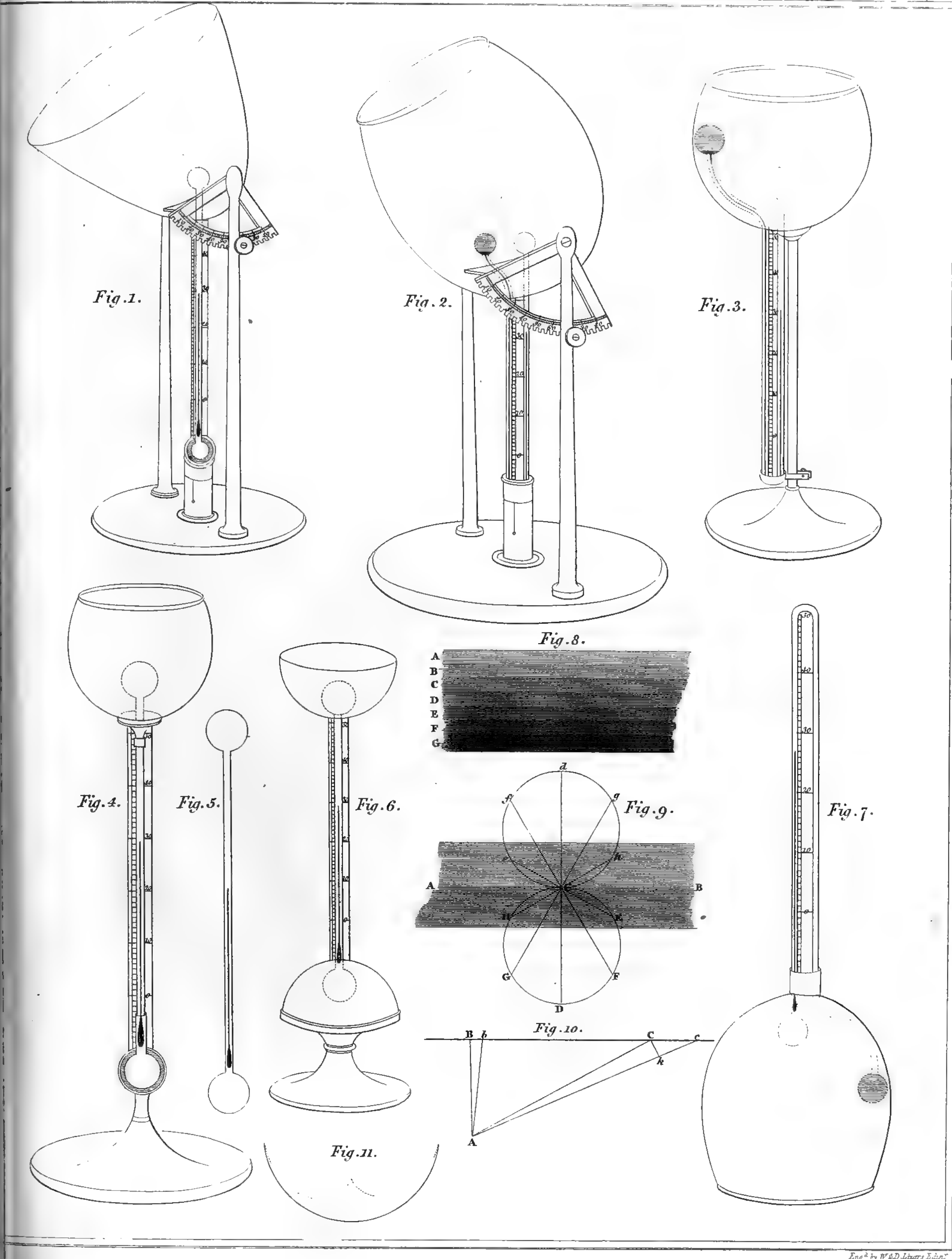

Engd by W.&D. Lizars Edinr

the accumulation of heat would sometimes hardly reach to three degrees. Under like circumstances, but especially when the air was still, the differential thermometer indicated more than twice as much effect on fresh ploughed land as on fine pasture. Nor was this inferiority of a surface of turf owing to the waste of heat produced by a more copious exhalation of moisture; for, on spreading a layer of dry hay, or even wool, over a part of the naked soil, the temperature of it was in a few minutes reduced to the same degree as that of the grassy sod.

The influence of reflex light, from the clouds and the sky, in heating the ground, is often very considerable. The rays sent from a fleecy canopy may sometimes amount to the half or the third part of those which would be received directly from the sun. But a stratum of dense black clouds intercepts almost the whole of the scattered light. A similar effect is produced by another sort of screen. Thus, when the sky was overcast, but the weather calm, the pendant differential thermometer intimated scarcely two degrees of heat at the surface, in a small fir wood; but marked eight or ten degrees, when carried to a neighbouring glade.

In this climate, about two hours after sunrise, the ground has the same temperature as the incumbent mass of air, but grows commonly warmer than it, till nearly two hours after noontide; from which time, it again declines, and becomes relatively colder than the air, perhaps two hours before sunset; sinking still lower during the night. The differences are thus comparatively very small between the temperature of the ground and that of the conterminous air, seldom exceeding the fifth, or perhaps even the tenth part of the whole diurnal change. The hot or cold pulses discharged from the ground, must, therefore, in all cases, be only trifling, and quite insufficient to produce that rapid approximation to an equilibrium which is actually observed. I

was

was hence still inclined to think, that the statement which I had formerly made, of the existence of an ascending warm current in the atmosphere, combined with a descending cold current, was adequate to the explication of all the phenomena. It is obvious, indeed, that the lower portions of air, becoming heated, in the progress of the day, by their contact with the ground, must rise upwards, and their place will consequently be supplied, by like portions of cold air which descend from the higher regions. These opposite currents, though set in motion during the flood of light, will continue their play long afterwards, perhaps through the greater part of the night, till again vigorously excited by the presence of the sun. Hence the ground is only heated to a certain limit during the day, and grows always colder as the night advances.

But my late ingenious and learned friend Dr Wells, by the publication of his Essay on Dew, which contains some acute observations, conjoined with a few striking experiments, though performed in the gross way, had contributed to revive the notion of a copious radiation, or pulsatory discharge of heat from the earth's surface. It was, therefore, desirable to ascertain the existence and real extent of such pulsations. The accurate means of determination were within my reach, though I had hitherto, in a great measure, neglected their application; for though the pyroscope measures those impressions with great delicacy in a close apartment, it is liable out of doors to some derangement from the influence of light, and has its action diminished by the sweep of violent winds. To avoid as much as possible these disturbing influences, I constructed a small pendant pyroscope, the lower ball being left naked, and the upper ball gilt with silver-leaf, to reflect almost the whole of the incident light. This instrument I fixed to a short arm, made to slide along a staff which could be stuck in the ground. In the month of August last, I carried it, with some other

other apparatus, into the country, for the greater convenience of making observations. This pyroscope being let down within a few inches of the ground, generally indicated an impression of heat during the day, seldom exceeding, however, three or four millesimal degrees. But I remarked, with surprise, in more than one instance, that towards evening, when the sky became clearer, the liquor would fall a degree or two. Yet the ground was still warmer than the air, and ought consequently to have augmented, rather than diminished, the calorific pulsation. I now began to suspect that an opposite impression was somehow showered from the atmosphere, and therefore set about examining the subject closely; though a tract of cloudy and boisterous weather greatly retarded my inquiries. I fitted, a little below the sentient ball of an ordinary pyroscope, a small circular plate of tin, hammered into a slight concavity. The instrument, having its action thus more than doubled, put the main fact beyond all doubt.

The object now was to discover, if those cold pulses shot downwards from an azure sky, were subject to variation, and whether they prevailed during the day, as well as at night. I had a segment of a sphere hammered out of tin, about nine inches wide, and three inches deep, with a vertical arch, to which the pendant pyroscope was attached, the sentient ball being placed in the middle between the centre and the bottom. But the indications were not altogether satisfactory, owing partly to the influence of strong light, though chiefly to the disturbing effects of the violent winds which happened then to prevail. I therefore resumed the erect pyroscope, and augmented its action, by adapting under the sentient ball an hemispherical tin-cup of about 2 inches in diameter. To screen the instrument from the sun and the wind, I placed it in the middle of a wide earthen pitcher set behind a north wall. The

weather

weather being very unsteady, I had to watch the intervals of an opening sky. Covering the mouth of the pitcher with a pane of glass, the pyroscope would sometimes indicate one or two degrees of heat; but, on removing that screen, it always marked impressions of cold amounting perhaps to five or six degrees, as often as the clouds parted, and left a blue space near the zenith. The continual darting, by day and night, of cold pulsations from the sky was thus ascertained; and nothing seemed wanting but to improve the construction of the instrument, and render it commodious and portable. I resolved to inclose both balls within the same cavity; the lateral ball being gilt, and of the same colour as that of the annexed reflector; and the naked sentient ball being placed in the focus. By this disposition, the influence of light was almost completely destroyed, its action being made equal on both the balls. But, to protect those balls from the disturbing effect of wind, it was requisite, that the cup containing them should be deep and rather narrow. If the cold pulses to be measured were darted only in a vertical direction, the parabolic conoid would certainly be the proper form of the reflector. I was convinced, however, from some imperfect trials, that such impressions were likewise sent down at different angles of obliquity. Supposing their intensity to be equal in all directions, an hemispherical reflector would answer best. But the focus, instead of a single point, would then have branched into the diverging arcs of the caustic curve, and the sentient ball of the pyroscope would have required half the dimension of the reflector itself, (see fig. 11. Pl. XI.) The main object, however, was to measure the impressions received from the upper portion of the atmosphere, that part near the horizon being generally obstructed by clouds or vapour. I therefore adopted an intermediate plan, and selected for the reflector a truncated oblong spheroid, cut through the upper focus by a plane perpendicular

pendicular to the axis, and having the sentient ball of the pyroscope placed in the lower focus, (see fig. 2, 3. and 4.) It is evident that the impressions which pass through the first focus in every direction, must be reflected towards the second focus. But since the upper focus occupies the middle of the aperture, all the cold pulses which enter the spheroid will be nearly collected on the sentient ball of the pyroscope. The more oblique impressions, however, striking the sides of the reflector at a greater angle of incidence, will be less copiously reflected; and consequently this compound instrument will indicate more fully the action of that quarter of the heavens to which it is turned.

Of these truncated spheroids, I had two made of planished tin; the one being eight inches high, with an extreme width of the same, and its focus two inches from the bottom; and another nine inches high, and six inches broad; the focus being only an inch from the bottom. I had one or two more reflectors, constructed of plated copper, after the same forms, but with only half those dimensions. The more eccentric spheroid being mounted on pivots, was better fitted to explore any particular portion of the sky, (see fig. 2. Pl. XI.) To this inquiry my attention was now drawn. Watching when the sky appeared most serene, I directed the instrument, first to the zenith, and then successively declined it, till it came within twenty degrees of the horizon. But the instrument continued to indicate the same frigorific impression, or about forty or fifty millesimal degrees at all inclinations. It was, therefore, ascertained, that the action of a given section or angular portion of the sky, is the same at every obliquity.

With the erect spheroid, I found, in cloudy weather, that the frigorific impression diminished, in proportion as the humid mass, floating in the atmosphere, seemed to descend. When the sky was canopied with high fleecy clouds, the effect on the

instrument might amount to twenty degrees; but when the congregated vapours sunk so low as to hover on the hilly tracts, the impression would frequently not exceed five degrees. It was evident, therefore, that the effect depends on the altitude of the lowest range of clouds, and might seem to result wholly from the difference of temperature which prevails there, compared with that of the surface.

But the same conclusion was drawn from another set of observations. In a calm day, when a mass of dark clouds was spread at no great elevation above the surface of the ground, the spheroid indicated only five millesimal degrees in a vertical position, and yet marked still the same quantity, when depressed to an angle of thirty degrees above the horizon. But had this impression of five degrees penetrated directly through the clouds, from the higher regions of the atmosphere, scarcely one-half of a degree could have escaped through the mass of vapours by the oblique passage. A range of clouds hence acts as a complete screen, absorbing and extinguishing all the hot or cold pulses received on it.

Since clouds consist merely of dispersed aqueous globules, their influence, I conceive, may be safely inferred from that of water itself. I therefore inclosed an inverted pyroscope in a spheroidal cup, and suspended it a few feet above the ground, while the sky appeared clear and blue, (see fig. 7. Pl. XI.); then passing a silver tray under it, the reflected impression of cold amounted to twenty-five degrees: on interposing a plate of glass, this was reduced to two degrees; but on removing it, and pouring a sheet of water over the silver, the effect was absolutely extinguished.

Most of these observations and experiments were made during the months of September and October 1817 on the top of the Tower at Raith,—a spot very favourable for such researches, and

and endeared to me by many pleasing associations. The result of them has been the construction of a delicate instrument, which will be deemed, I hope, a valuable accession to meteorology, and indeed to physical science in general. From the term *αιθριος*, which, in reference to the atmosphere, signifies at once *clear*, *dry* and *cold*, I have appropriated the name of *Æthrioscope* to this new combination of the pyroscope. The sensibility of the instrument is very striking, for the liquor incessantly falls and rises in the stem with every passing cloud. Under a fine blue sky, it will sometimes indicate a cold of 50 millesimal degrees; yet on other days, when the air seems equally bright, the effect is only 30°. The causes of these variations are not quite ascertained. The action is in general greatest under a clear and translucid atmosphere. But particular winds, blowing at different altitudes, seem to modify the effect, and so may perhaps the transition from summer to winter.

There are three principal forms of the Æthrioscope: 1. Erect; 2. Sectoral; 3. Pendant.

1. *Erect.*—All the requisite conditions for measuring the atmospheric impressions are attained, by adapting the pyroscope to the cavity of a polished metallic cup, of rather an oblong spheroidal shape, the axis having a vertical position, the lower focus being occupied by the sentient ball, while the section of a horizontal plane, at the upper focus, forms the orifice, (see fig. 3. Pl. XI.) The cup may be made of thin brass or silver, either hammered or cast, and then turned and polished on a lathe; the diameter being from two to four inches, and the eccentricity of the elliptical figure varied within certain limits according to circumstances. The most convenient proportion, however, is, that the eccentricity should be equal to half the transverse axis, or that the focus should be placed at the

third part of the whole height of the cavity, the diameter of the sentient ball having likewise nearly the third part of the diameter of the orifice of the cup. In order to separate the balls of the pyroscope, the gilt one may be carried somewhat higher than the other, and lodged in the swell of the cavity, its stem being bent to the curve, and the neck partially widened, to prevent the risk of dividing the coloured liquor in carriage. The instrument is adjusted to its position by a clasp which slides along the stalk. A lid of the same thin polished metal as the cup itself, is fitted to the mouth of the æthrioscope, and removed only when an observation is to be made. The scale may extend to 60 or 70 millesimal degrees above the zero, and about 15 degrees below it.

2. *Sectoral.*—To ascertain and measure the cold pulses shot obliquely, as well as those in the vertical direction, the æthrioscope may be constructed to turn towards any portion of the sky, (see fig. 1. and 2. Pl. XI.) To effect this, the form best fitted for reflexion would be that of an hyperbola, whose asymptotes have an inclination equal to the visual angle of the space to be explored. Fig. 1. may give an idea of the construction. But to obtain accurate results, the focal ball must be small, and the hyperbolic conoid wide and much extended. It will answer nearly the same purpose, however, to adopt a truncated spheroid, of great eccentricity. Let the height of the focus, for instance, be one inch, that of the entire cavity nine inches, and, consequently, the widest diameter six inches. The shape represented in fig. 2. is rather more distended, its extreme width being equal to double the eccentricity, and the focal ball dividing the height of the orifice in the ratio of 1 to $3 + \sqrt{8}$, or of 6 to 35 nearly. The pyroscope inserted has a peculiar twisted form, and receives its adjustment from a moveable socket. While the sentient ball remains always in the same position, the

the axis of the instrument can, by means of a screw acting on the limb of a quadrant, be depressed or elevated to any given angle. But the effect will chiefly be produced by the direct impressions: for the lateral pulses, striking less obliquely against the cavity of the spheroid, will be feebly reflected.

This moveable æthrioscope was placed in a convenient situation out of doors, when the sky appeared free from clouds, and had assumed a clear blue tint. The spheroid being turned first upright, the effect was noted; but this continued still unchanged, on depressing the axis successively, till it had approached the limit of energetic range, or within 20 degrees of the horizon.

From every portion of the sky that subtends a given visual angle, there is hence received the same quantity of the frigorific pulses. But such would likewise be the result, if they were showered from the horizontal surfaces of the successive strata which divide the atmosphere; since, although the intensity diminishes in the ratio of the sine of obliquity, a propellent space broader in proportion is, for each elemental angle, brought into action, as appears from the inspection of Fig. 10. Let BC represent the boundary from which hot or cold pulses are darted: A being the point at which these are received, let B*b* and C*c* denote minute portions of the vibrating surface. A perpendicular C*k* to AC would send impressions in the direction CA, equal to those emitted by C*c*. But that the effects produced at A, from the projections of C*k* and B*b* should be equal, the distances CA and BA must be proportional to those breadths; wherefore the triangles CA*k* and BA*b* are similar to the elemental angles CA*c* and BA*b* equal. It hence follows, that the impressions sent from a cluster of such angles, amounting perhaps to ten or fifteen degrees, near the vertical position, are equal to those contained within the same aggregate angle in an oblique direction. This entire

agreement

agreement between theory and observation is most satisfactory.

3. *Pendant.*—The æthrioscope might be reduced to a smaller and more compact form, by conjoining with it a pendant differential thermometer. Neither of the glass balls in this case requires to be gilt. But of the Pendant Æthrioscopes there are two varieties. In the *first*, the balls of the pyroscope are enclosed by hemispherical brass cups. (See Fig. 6. Plate XI.) These, however, may be a little deeper, forming segments about 10 or 15 degrees more than the hemisphere. The pyroscopic balls, with two-fifths of the diameter of the cups, should be placed in the middle, between the centre and the bottom. In the *second* variety, the lower ball of the pyroscope is encased by a hollow sphere of brass, composed of two pieces which screw together, and the upper ball occupies the focus of the cup, which needs scarcely be more than two inches wide. (See Fig. 4.) This variety of the instrument is more portable than any of the rest, and equally accurate; but owing to the brass casing, it is, under a change of temperature, rather slower in its action. The construction is rendered still more commodious, by having the stem of the differential thermometer inserted through a projecting circular piece, somewhat larger than the upper ball, which screws into a perforation at the bottom of the cup, and then forms part of the same reflecting cavity. This arrangement allows not only the standard of the instrument to be occasionally detached, but also the cup itself, —a circumstance at once conducive to the safety of carriage, and to the preservation of the metallic lustre and polish.

On replacing the metallic lid, the effect is entirely extinguished, and the fluid in the stem of the differential thermometer immediately sinks to zero. A cover of pasteboard has at first precisely the same influence; but after it has itself become chilled by this exposure, it produces a small secondary action

tion on the sentient ball, scarcely exceeding, however, the tenth part of the entire and original impression. A lid of glass or of mica intercepts the impressions like one of paper; for the admission of light has no deranging effect, if the æthrioscope be rightly constructed and highly polished. The minute secondary action is almost extinguished, if screens of paper, glass, or mica, be held at some distance above the mouth of the instrument.

The variety composed of two hemispherical cups will answer, as an inverted æthrioscope, for measuring, at some elevation, the warm pulses sent up from the lower strata. It is only requisite to cover the upper hemisphere during the observation with its metallic lid. The same form of the instrument might likewise conveniently be employed, when its altitude is not very considerable, to determine the difference of the temperature of the surface of the earth, or of the sea, from that of the superincumbent stratum of air. This difference, it would appear, from some unfinished observations which I have made, is expressed, on Fahrenheit's scale, by two-thirds of the millesimal degrees indicated by the compound pyroscope. Nor is this effect sensibly altered by the proximity of the terminating surface, because its indefinite expansion will always present nearly the same visual angle. Hence the relative temperature of the surface of the sea, may be easily discovered from an æthrioscopical observation performed at the stern or the prow of a ship while under full sail.

In the Pendant Æthrioscopes, both the glass balls are left naked; but, in the Erect and Sectoral kinds, the lateral ball is always gilt. This condition, however, is not essential, since the concentration at the focus would be sufficient, by its excess alone, to produce an adequate effect. Hence the observations may be varied, by introducing within the reflector a differential thermometer, either consisting of translucid balls, or composed of balls blown from black glass. When the latter kind is used, the

the liquor will indicate merely the difference of opposite effects, and will rise or fall according as the impressions of cold or heat sent from the sky chance to predominate. If the light reflected down from the heavens be profuse, it will excite more heat than the simultaneous frigorific impressions can destroy. Under a canopy of fleecy clouds, a considerable excess of heat is hence excited on the black ball; but when the sky is clear, the influence of cold generally prevails, increasing as the sun declines.

The question is now to discover the cause of the phenomena which have been thus revealed. I have already stated, that different experiments appeared to concur in indicating the coldness of the superior atmosphere as the source of those effects. Since pulses are darted from such various surfaces, and since the softness of the external coat, and its tendency to fluidity, seem vastly to augment their power; may they not likewise be excited from a boundary of air itself? This extension of a great principle in the economy of Nature, has never yet been surmised; nor can it be readily brought to the test of direct experiment, since a body of air, whether hotter or colder than the general medium, would evidently not remain stationary, but continually rise or fall.

I sought accordingly to examine the effect of directing the æthrioscope to a hot stream of ascending air. I placed on bricks before that instrument, the lower part being screened, a large mass of iron carried from the fire, at almost a red heat. The æthrioscope then gave impressions of heat or cold, according as its aperture was without or within the warm current, or was affected by the anterior or the posterior boundary. But this experiment proved very troublesome, and occasionally turned out a little unsatisfactory. Another experiment performed out of doors, to try the action of hot smoke raised from

a train of damp straw set on fire, was, from the difficulty of managing it, found to lead to no certain conclusion.

But during winter, a much easier mode occurred for deciding the question. In a room where a steady fire was kept up, the sectoral æthrioscope was set on the inside of the window, and directed to the upper part of the opposite wall; but on throwing up the window, the instrument being now surrounded by a body of cold air, which, however, did not penetrate far into the room, the liquor sank 5 or 6 degrees, indicating impressions of heat, caused evidently by the excess of temperature of the remote air of the room above that which was contiguous to the æthrioscope. It need hardly be observed, that the effect increased in colder, and diminished in milder, weather.

A similar experiment is readily performed by help of the erect æthrioscope. In a close apartment, where a good fire is constantly kept up, the ceiling and the floor may be discovered by the pendant differential thermometer to have exactly the same temperature with its adjacent stratum of air. Yet the upper portions of the confined air of the room will be found several degrees warmer than the lower. Instead of being divided only into opposite ranges, the whole mass, from the floor to the ceiling, will, in consequence of the expansion and buoyancy of its heated particles, form a series of intermediate strata, not distinguished, however, by any very precise boundaries. But the intensity of action being proportional to the difference of temperature, the effect on the æthrioscope must evidently be the same, whether it is produced by a single set of large pulses or by several sets of smaller ones. If, for example, instead of one bounding surface, above which the air is seven degrees warmer than immediately below it, we suppose seven such boundaries, each having an excess of temperature of only a degree: the pulses excited at the first of these intermediate surfaces, and successively augmented as they reach the second, third, and

 fourth,

fourth, &c. surfaces, will at last acquire the same energy as if the aggregate difference of seven degrees had been all exerted at once. Thus, the under surface of the stratum G (see fig. 8. Pl. XI.) darts pulses downwards, which, being augmented in succession at the under surfaces of the strata F, E, D, C, B, and A, may have finally the same intensity as if they had originated from the opposition of the extreme strata G and A. Accordingly, having planted a large screen immediately before the fire, and placed a delicate pyroscope about the middle of the room, with a broad circular piece of metal suspended a few inches above it; on withdrawing this canopy after some time, the instrument indicated a small impression of heat, seldom exceeding, however, one degree. But the effect may be rendered more sensible, by a moderate concentration of the power excited. Thus, the hemispherical pendant æthrioscope (fig. 6.) will, in the same situation, mark a very sensible calorific impression, amounting, at least in ordinary cases, to three or four degrees. Hot pulses are, therefore, actually shot downwards from all the upper strata of the confined air of a room in which a fire is kept steadily burning.

The experiment can be likewise reversed. Let an inverted æthrioscope, composed of a pendant differential thermometer, have its sentient ball fitted with a small hemispherical cup which is turned downwards, (see fig. 7.) This instrument being set on the floor, will remain at zero; but if lifted only a few feet, it will indicate a visible impression of cold received from below, which will increase to three or four degrees when the æthrioscope is suspended near the top of the room. Wherefore, the upper surfaces of the successive decumbent strata, being comparatively colder, send upwards a series of chilling pulsations. Each of the conterminous boundaries appears thus to perform a double operation, shooting downwards impressions of heat, and darting upwards equal impressions of cold. Such a mutual exchange

exchange of influence must evidently tend to accelerate that progress to an equilibrium which the gradual intermixture of the different strata, if left quite undisturbed, would in time produce. The air of a close apartment, exposed to the action of a steady fire, is hence kept agitated through its whole mass by a series of opposite tremors, which continually disperse, in all directions, the irregularities of temperature.

If the action of the pulses excited in the air of a small room be made thus apparent, how much more striking should we expect to find the effect produced by the mingled tide of commotion collected from the vast body of the atmosphere itself? Taking even the lowest range of strata, to the height perhaps of two miles, including scarcely one-third part of the whole aërial mass, the difference of temperature between its extreme boundary will amount to 20 centesimal degrees, or 36 on Fahrenheit's scale. The order of the series, however, is exactly the reverse of what takes place in a close room, the air of the superior regions being invariably colder than at the surface of the earth. Accordingly, the simple pyroscope, exposed in calm weather to a clear and open sky, will, at all times, if not disturbed by the influence of a strong light, indicate large impressions of cold, amounting to 5 or perhaps even 10 degrees. In most cases, it may be sufficient to screen this instrument from the direct action of the sun's rays. But the action of light will be almost neutralized, by opposing a diaphanous ball to one gilt with silver, or contrasting a ball of the different shades of green or blue, to another coated with pure gold leaf. But to procure consistent results, it is still more necessary to guard against the deranging influence of winds.

The same sectoral form of the æthrioscope discloses also the peculiar influence of clouds in obstructing the frigorific pulses excited in the atmosphere. When the sky was completely obscured by a dense canopy of clouds, the instrument being

 pointed

pointed to the zenith, marked only five millesimal degrees; but, on lowering it successively to the angle of 30 degrees above the horizon, it continued to indicate still the same effect. Water almost completely absorbs the pulsatory impressions of heat or cold; and may not clouds, consisting of diffuse aqueous particles, produce a similar effect? But the feeble action of five degrees, amounting scarcely to the eight part of what is observed in clear weather, could not be any remnant of the pulses from the higher celestial regions, which had penetrated through the mass of vapours; because, if the vertical transit, through the obstructing range, allowed only an eighth part to escape, the oblique passage of 30 degrees, redoubling the extent of absorption, would have reduced the final discharge to five-eighths of a degree. The impression measured by the æthrioscope, in this case, must therefore have originated wholly in the strata of air between the under surface of the clouds and the ground. But in that narrow space, the extreme difference of temperature would be comparatively small. Hence the frigorific action is found always to diminish as the clouds descend. Nor does their variable denseness appear materially to affect the result, which is often the least, when a very thin, whitish, but low vapour, gathers in the atmosphere. Hence the æthrioscope might, with great facility, be employed in estimating the altitude of clouds.

As the higher strata of the atmosphere thus dart cold pulses downwards, so the lower strata must evidently project equal pulses of heat upwards. But to measure these, it would require, as in fig. 7. the æthrioscope to be inverted and furnished with a pendant differential thermometer. The instrument, now carried to the top of a lofty mountain, and directed to the plain below, would indicate a considerable impression of heat, nearly proportional to the quantity of ascent; and, therefore, amounting, for example, on the summit of Chimboraço, to perhaps 20 millesimal

millesimal degrees. But, in the same situation, the common æthrioscope might be expected to mark an impression of cold from above, as just so much diminished. No opportunity, however, has yet occurred, on a large scale, for making these interesting observations. The ascent of a balloon would afford the readiest mode of verifying and extending the theory.

The nature and intensity of the cold and hot pulses excited in the several strata of the atmosphere, may be easily understood from (fig. 9. Pl. XI.) Let two equal and opposite circles touch the straight line AB, which divides a stratum of cold, from another of warm, air. While the opposite diameters CD and C*d* represent the forces of the perpendicular pulses of cold darted downwards, and of heat shot upwards, the chords CE, CF, CG, and CH, and C*e*, C*f*, C*g*, and C*h*, will likewise exhibit the strength of the pulses which are transmitted with various obliquity.

The inverted æthrioscope likewise discovers the quality and measure of the pulses projected from the ground. These, in general, are very feeble, seldom in this climate exceeding three or four degrees. In the progress of a bright day, as the ground grows warmer than the incumbent air, it excites hot pulses; but, as the sun declines, the effect gradually diminishes; till this again returns, increasing with a contrary character, when the surface of the earth has become relatively colder.

The same instrument being suspended a few feet above the ground while the sky appeared clear and blue, a silver tray was laid upon it, and the reflected impression of cold amounted to 25 degrees; but, on interposing a plate of glass, it was reduced to two degrees; and on removing this, and pouring a sheet of water over the silver, the effect was absolutely extinguished. The absorbent influence of water, and consequently of clouds, was thus distinctly shown.

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The æthrioscope thus opens new scenes to our view. It extends its sensation through indefinite space, and reveals the condition of the remotest atmosphere. Constructed with still greater delicacy, it may perhaps scent the distant winds, and detect the actual temperature of every quarter of the heavens. The impressions of cold which arrive from the north, will probably be found stronger than those received from the south. But the instrument has yet been scarcely tried. I am anxious to compare its indications for the course of a whole year, and still more solicitous to receive its reports from other climates and brighter skies *.

All those effects are no doubt more conspicuous in the finer regions of the globe. Accordingly, they did not escape the observation of the ancients, but gave rise to opinions which were embodied in the language of poetry. The term Αηρ was applied only to the grosser part of the atmosphere, while the highest portion of it, free from clouds and vapour, and bordering on the pure fields of æther, received the kindred appellation of Αιθρια. But this word and its derivatives have always been associated with the ideas of *cold*. The verb εξαιθριαζω is adopted by Athenæus, to signify the cooling of a body by mere exposure

* In this stage of the inquiry, it may be proper to notice a singular observation, which I have not yet had an opportunity of repeating. An æthrioscope exposed to the free air, on a platform projecting towards the north, from the window of my experimental room in Queen Street, stood a few weeks since, in a clear frosty day, at about 25 degrees; but, on the approach of evening, a light wind having suddenly turned to the opposite point of the compass, the atmosphere became at once obscured by a body of very thick and dark smoke: the liquor, contrary to all expectation, immediately rose more than 10 degrees, and remained stationary till the dense mass again disapersed. This stratum of fuliginous matter would no doubt absorb the frigorific impressions showered from the sky; yet being precipitated by its collected weight, it would bring down intense coldness from the superior regions, and therefore dart new impressions, rendered the more powerful from the proximity of their source.

exposure under a serene sky. Homer uses the term Αιθρος, in speaking of the reception of his hero, when overcome with *cold* and toil *. The same graphic poet applies the epithet Αιθρηγενης or Αιθρηγενέτης, or *frigorific*, to Boreas, the north wind †. The Chorus, in the *Antigone* of Sophocles, deprecates alike the pelting storm and the cold (αιθρια) of inhospitable frozen tracts ‡. The word αιθριος is employed by Herodotus, to signify a chill, as well as a dry, atmosphere ‖. Of the same import is the expression in Horace—*Sub Jove frigido.*

But the facts discovered by the æthrioscope are nowise at variance with the theory that regulates the gradation of heat from the equator to the pole, and from the level of the sea to the highest atmosphere. The internal motion of the air, by the agency of opposite winds, still tempers the disparity of the solar impressions; but this effect is likewise accelerated by the vibrations excited from the unequal distribution of heat, and darted through the atmospheric medium with the celerity of sound. Any surface which sends a hot pulse in one direction, must evidently propel a cold pulse of the same intensity in an opposite direction. The existence of such pulsations, therefore, is in perfect unison with the balanced system of aërial currents.

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* Αἴθρῳ και καμάτῳ δεδμημένον ἦξεν ἐς οἶκον. *Odyss.* Lib. xiv. 318.

† Ὡς δ' ὅτε ταρφειαὶ νιφάδες Διὸς ἐκποτέονται,
Ψυχραὶ ὑπαὶ ῥιπῆς αἰθρηγενέος Βορέαο. *Iliad.* Lib. xix. 357–8.
Καὶ Βορέης αἰθρηγενέτης, μέγα κῦμα κυλινδων. *Odyss.* Lib. v. 296.

‡ Δυσαύλων πάγων αἴθρια
Καὶ δυσομβρα φευγειν βέλη. *Antigone*, 357.

‖ Θερμότεροι γαρ δη εστι το ὕδωρ τῆς τε αἰθριας και τῆς δροσυ. *Euterpe.*

XXII. *A Method of determining the Time with Accuracy, from a Series of Altitudes of the Sun, taken on the same side of the Meridian. By* Major-General Sir THOMAS BRISBANE, Knt. F. R. S. E.

(Read Feb. 2. 1818.)

HAVING for a number of years been constantly moving about, in situations where I could not convey large astronomical instruments, I have repeatedly tried to what extent of accuracy and consistency I could arrive with the smaller ones. The results have convinced me, that a great deal of accuracy may in that way be obtained; and that the sextant is an instrument, which, if perfectly understood, would be in higher estimation, and more general use, than it is at present. The observations I am about to submit, were made with a ten inch sextant of Troughton's, divided on platina to 10″, (No 1200). The manner of using it, which I am now to describe, is what I have pursued for a great length of time, both at sea and at land, and I can recommend it as uniting simplicity with accuracy, and at the same time as serving to discover errors, if they happen to exist. Our climate does not admit of obtaining equal altitudes very frequently, but I conceive that the mode which I wish to see adopted, will admit of equal accuracy, and I am justified

justified in this conclusion, by the results of many hundred trials, verified by calculating as simple altitudes those from which I had deduced the time as equal altitudes. Indeed I am rather disposed to give the preference to the simple altitudes for accuracy, if a considerable change of temperature has taken place between the morning and evening observations, as the consequent change of refraction is seldom taken into account in the calculation of the equations to equal altitudes, though it may produce a sensible effect on the determination of the time. To overcome these difficulties, has been my sole object, and my only wish, in making this communication, is to enable every observer to put this method to the test, in the hope that he may derive the same satisfaction from it that I have done.

In the morning, when the sun has nearly 10° of altitude, and farther from noon than two hours, if far advanced in the season, but otherwise, the nearer the prime-vertical the better, I observe eleven successive altitudes of the sun's lower limb, reflected from quicksilver, where the situation will admit of it; where it did not, I have employed with equal success pure limpid oil covered by one of Troughton's plate-glass angular roofs; but in most situations it may be used in a room, without any cover; always taking care, in the morning observations, to set the index, with the utmost accuracy, to an even 10′ or 20′ greater than the sun's altitude, and then to wait the contact. If I use a chronometer, I have an assistant, who counts the seconds aloud, which I direct him to note, with the fractions, &c. at each observation. In the afternoon I proceed in the same way, only setting the sextant 10′ or 20′ less than the sun's altitude, and carefully noting the barometer and thermometer, for each series of observations, in order to correct the mean refraction.

I send you a type of the calculation complete, in order that any one who wishes to pursue it, may easily be enabled to put it in practice. As the altitudes are all successive, the intervals ought to be nearly equal; by which means, merely casting the eye over the results, you readily discover any inconsistency, if it should exist. Although there may appear a number of figures in the work, it is extremely simple, as I have only to combine the error of the instrument with the refraction, parallax, and semi-diameter of the sun, for the first and last observation, the tenth part of which difference must be equally distributed throughout the series, in order to obtain each altitude; and the same system applies to the rest of the work. As the altitudes increase or diminish by a uniform quantity, their natural sines are all taken out at the same opening of the book, and the proportional parts for seconds, applied as appears in the mode I have adopted, and have herewith transmitted. The Table entitled *Logarith. Rising*, in the Requisite Tables, although perfectly equal to all nautical purposes, I did not consider as sufficiently extended to give the tenths of seconds, which modern instruments and Logarithmic Tables afford the means of arriving at. I therefore computed a Table entirely a-new, from the formula

$$2 \sin.^2 (\tfrac{1}{2} a) = \frac{\sin.(\text{co. lat.} + \text{dec.} \odot) - \sin.(\text{alt.} \odot)}{\cos.\text{ lat.} \times \cos.\text{ dec.} \odot};$$

but taking the logarithms from tables to ten places of figures, although only retaining eight for those in the computation, as being sufficient to give me the last figure correct. These Tables begin at 2 h. from Noon, and are carried on to 6 h., and to every 10″, with the differences corresponding. The results were proved by the first and second differences, and lastly, by comparing the first five figures with those given in the Requisite

Requisite Tables. The Table of natural sines given in the Appendix to that work, is most convenient for the calculation of the time by this method. If this communication is thought worthy of a place in the *Transactions of the Society*, of which I have the honour to be a member, I shall have the satisfaction of making another, respecting the mode of determining latitudes by the sextant most correctly, by a series of observations made near noon. I should also wish to submit a paper on the Repeating Circle, and the extraordinary accuracy of the results I have obtained from observations made with a circle of Troughton's, twelve-inch in diameter, if the Society shall think the matter deserving of its attention.

Paris, 23d November 1817.

The

The following are the Times and Altitudes of the Morning and Evening Observations, from which the Calculations of the Times from Equal and Simple Altitudes have been deduced, as referred to in the respective Calculations.

8h 41′ 49″.7	23° 10′	2h 56′ 39″.3
- 42 34.3	- 20	- 55 54.3
- 43 17.3	- 30	- 55 10.3
- 44 1.3	- 40	- 54 25.0
- 44 46.3	- 50	- 53 41.4
- 45 30.3	34 0	- 52 58.3
- 46 16.3	- 10	- 52 11.3
- 47 1.3	- 20	- 51 26.0
- 47 45.3	- 30	- 40 42.0
- 48 29.3	- 40	- 49 57.0
- 49 15.3	- 50	- 49 12.0
- 50 1.3	25 0	- 48 26.4
- 50 46.3	- 10	- 47 40.7

Barometer, Morning Observation, 748.30
Thermometer, do. - 10.3

Barometer, Evening Observation, 745.60
Thermometer, do. - 14.00

Calculation of Time, from Equal Altitudes.

⊙ Longitude 7h 14° 7′

8h 41′ 49″.7	34″.3	17″.3	1″.3	46″.3	30″.3	16″.3	1″.3	45″.3	29″.3	15″.3	1″.3	46″.3
2 56 39.3	54.3	10.3	25.0	41.4	58.3	11.3	26.0	42.0	57.0	12.0	26.4	40.7
11 38 29.0	28.6	27.6	26.3	27.7	28.6	27.6	27.3	27.3	26.3	27.3	27.7	27.0
	14.3	13.8	13.2	13.8	14.3	13.8	13.6	13.7	13.1	13.7	13.8	13.5

40 14.5	
14.3	
13.8	
13.2	
13.8	
14.3	
13.8	
13.6	
13.7	
13.1	
13.7	
13.8	
13.5	
49.1	
11 49 13.77	11 49 13.77
	+ 17.68
	11 49 31.45

8h 46′ m. Time, Morning Observations.
14 52 m. Time, Evening Observations.

6 06 Interval.
3 03 ½ Interval.

CALCULATION FOR FINDING THE TIME WITHOUT EQUAL ALTITUDES.

Chateau de Sainte. Pas de Calais. Morning Observations. Very Clear. ♀ 7th Nov. 1817, Lat. 50° 19′ 57″ Long. 2″ in Time west of Paris. Error of Inst. — 6′

8 41 49.7	42 34.3	43 17.3	44 1.3	44 46.3	45 30.3	46 16.3	47 1.3	47 45.3	48 29.3	49 15.3
23 10.0	23 20	23 30	23 40	23 50	24 0	24 10	24 20	24 30	24 40	24 50

— 6 Error.
23 04 0

23 04 0
11 32 0
\+ 11 45.5
11 43 45.5

8 41 50 / 10 34 / 8 52 24 / 3 07 36

8 49 15 / 10 34 / 8 59 49 / 3 00 11

Daily var. ⊙'s decl. 17 55 = 3.0314085 | 3.0314085
3 7 36 — 4.0513841 | 4.0338659
24 — 5.0634863 | 5.0634863
3.1462789 | 2.1287607
140 0 | 134 5

Error. — 6
24 44
12 22
\+ 12 3 6
12 34 3 6

Ther.	10.3	9.9995		9.9995
Bar.	748 30	9.9931		9.9931
3 D.	- 78	2.4280	77	2.3936
	28	172	38	218
		2.4378		2.4080

274 1 Refrac. 255.9
8 7 Paral. 8.6
265 4 Ref. – 247.3
— 4 25 4 Par. + 4 7.3
⊙'s semi-diameter, + 16 10 9 16 10.9
\+ 11 45 5 + 12 03 6

11 43 45.5 11 48 47.3 11 53 49.1 11 58 50 9 12 3 52.7 12 8 54.5 12 13 56.4 12 18 58 12 23 59.9 12 29 1.8

⊙'s declin. first obser. 16 14 17 co-sine 9.9823202 ⊙'s declin. last obser. 16 14 22 5 Co-sine, 9 9823168
Co-ar. co-sine, co-lat. 50 19 57 0.9149539 - - - - - - - - 9·1949539
0.2126337 0.2126371

⊙'s declin. at noon, 16 16 37 16 16 37
3 7 36 = 2 20 3 0 11 = 2 14.5
16 14 17 16 14 22.5

⊙'s declin. first obser. 16 14 17 Last observ. 16 14 22.5
Co Latitude, 39 40 3 39 40 3
⊙'s meridian alt. = 23 25 46 Nat. sine 397619 23 25 40.5 Nat. sine 397594 Differ. 25

⊙'s mer. alt. 1st obser. 23 25 46 Nat. sine	397619	397617	397614	397612	397609	397607	397604	397602	397599	397597	397594
Nat. sines of alt. 11 43 45 5	203072	204496	205920	207343	208765	210187	211609	213030	214451	216156	217575
	194547	193121	191694	190269	188844	187420	185995	184572	183148	181441	180019
Prop. parts for seconds of alt. 45 5 =	216	225	233	243	251	259	269	276	285	8	17
	194331	192896	191461	190026	188593	187161	185726	184296	182863	181433	180002
Log. - - - -	9.288542	9.285323	9.282080	9.278813	9.275526	9.272215	9.268873	9.265522	9.262126	9.258716	9.255277
Co-ar. co-sine, co-lat. ⊙'s declination,	0.212634	212634	212634	212634	212635	212635	212635	212636	212636	212636	212637
Horary, - - -	9.501176	.497957	.494714	.491447	488161	484850	481508	478158	474762	471352	467914
	0975 727)201(2.8 1454 556										471 761)443(5.8 3805 625
	3 7 42.8	3 6 58.6	3 6 14.3	3 5 29 9	3 4 45.5	3 4 0.9	3 3 16.1	3 2 31.3	3 1 46.2	3 1 1.1	3 0 15.8
	8 52 17.2	8 53 01.4	8 53 45.7	8 54 30.1	8 55 59.1	8 55 59.1	8 56 43.9	8 57 28.7	8 58 13.8	3 58 58 9	8 59 44.2
	8 41 49.7	42 34.3	43 17.3	44 1.3	44 46.3	45 30.3	46 16.3	47 1.3	47 45.3	48 29.3	49 15.3
	10 27.5	10 27.1	10 28.4	10 28.8	10 28.2	10 28.8	10 27.6	10 27.4	10 28.5	10 29.6	10 28.9

10 27.5
27.1
28.4
28.8
28.2
28 8
27 6
27 4
28.5
28 9
81.2 Mean E. A. T. = 10′ 28″.12

Calculation for the Evening Observations

Times, -	2 49 12 0	49 57 0	50 42 0	51 26 0	52 11 3	52 58 3	53 41 4	54 25 0	55 10 3	55 54 3	56 39 3
Altitudes, -	24 50	24 40 0	24 30 0	24 20 0	24 10 0	24 0 0	23 50 0	23 40 0	23 30 0	23 20 0	23 10 0
	Er. — 6										Error — 6

First observation:
½ 24 44 0 ⊙'s daily var. 17 39 Log. = 3.0253059
12 22 0 — 2 59 41 — 4.0326590
24 — 5.0634863
2.1214512
132 3 = 2 12 3

17 39 Log. = 3.0253059
3 7 8 — 4.0503024
24 — 5.0634863
2.1390946
137 8 = 2 17 8

12 22 0
\+ 12 8 2
12 34 8 2

Last observation:
23 04
11 32 0
11 32 0
\+ 11 50 3
11 43 50 3

Therm. 14 0 = 9.9934
Bar 745 60 = 9.9916
3 D. 77 = 2.3936
38 = 218
2.4004
251 4
8 7
242 7
— 4 2 7
\+ 16 10 9
12 8 2

9.9934
9.9916
78 = 2.4280
28 = 172
2.4302
269″ 3
8 7
260 6
Refrac. Paral. — 4 20 6
⊙'s semidiam. + 16 10 9
11 50 3

Altitudes corrected,	12 34 8 2	12 29 6 4	12 24 4 6	12 19 2 8	12 14 1 0	12 8 59 2	12 3 57 5	11 58 55 7	11 53 53 9	11 48 52 1	11 43 50 3

⊙'s declination, 1st observation, = 16 18 49 Co-sine = 9.9821530 Last observation, 16 18 55 Co-sine = 9.9821493
Co-latitude, co-sine, ar. co. 39 40 08 — 0.1949539 - - - - - - 0.1949539
0.2128009 — 0.2128046

⊙'s declin. 1st observation, 16 18 49 Last observation, 16 18 55 cosine
Co-latitude, - - 39 40 03 - - - 39 40 03
23 21 14 N. sine 396409 — 23 21 08 Nat. sine = 396383

⊙'s mer. alt. 23 21 14 Nat. sine	396409	396406	396403	396401	396398	396395	396393	396390	396388	396385	396383
12 34 8	217575	216156	214735	213315	211893	210187	208765	207343	205920	204496	203072
	178834	180250	181668	183086	184505	186208	187628	189047	190468	191889	193311
Proportional parts, - -	39	30	17	13	5	281	272	265	257	247	239
	178795	180220	181651	183073	184500	185927	187356	188782	190211	191642	193072
Logarithm, - - -	9.252355	9.255803	9.259238	9.262624	9.265996	9.269343	9.272668	9.275961	9.279236	9.282491	9.285719
Ar. co. co-sine, co-lat. — sun's decl.	0.212801	0.212801	0.212802	0.212802	0.212803	0.212803	0.212803	0.212804	0.212804	0.212805	0.212805
	9.465156	9.468604	9.472040	9.475426	9.478799	9.482146	9 485471	9 488765	9.492040	9.495296	9.498524
	4418										058
	764)738(9 7										730)466(6 4
	6876										4380
	5040										280
	2 59 39 7	3 0 24 9	3 1 10 2	3 1 55 0	3 2 39 9	3 3 24 6	3 4 9 2	3 4 53 6	3 5 37 9	3 6 22 8	3 7 6 4
	2 49 12 0	2 49 57 0	2 50 42 0	2 51 26 0	2 52 11 3	2 52 58 3	2 53 41 4	2 54 25 0	2 55 10 3	2 55 54 3	2 56 39 3
	10 27 7	10 27 9	10 28 2	10 29 0	10 28 6	10 26 3	10 27 8	10 28 6	10 27 6	10 28 0	10 27 1

10 27 7
27 9
28 2
29 0
28 6
26 3
27 8
28 6
27 6
28 0
27 1
11)306 8
10 27 88

E. — Apparent Time, -	= 10′ 27″ 88
E. — by mean morning observation,	10 28 12
Mean of both, - - - - -	10 28 00
Apparent noon by simple altitudes, - - - - -	11 49 32 00
Ditto by equal altitudes, same day, - - - - -	11 49 31 45
Difference from equal altitudes,	+ 55

CALCULATION FOR THE TIME WITH AND WITHOUT EQUAL ALTITUDES.

Valenciennes. ♃ 25th September 1817.

Mode of determining the Time by a Sextant of TROUGHTON'S, No. 1200. Valenciennes, 25th September. Very clear.

Latitude 50° 21′ 15′ Longitude East of Paris 4′ 44″ in time.

Times, -	8h 1′ 52″.8	2′ 28″.2	3′ 3″.8	3 39.3	4 18.4	4 48.3	5 24.3	5 59.3	6 36.3	7 11.3	7 46.3
Altitudes observed,	38° 10′	38° 20′	38° 30′	38° 40	38° 50	39° 00′	39° 10′	39° 20′	39° 30′	39° 40′	39° 50′
	6										6

	2)38 04
	19 02
Correction for Refr. Parallax,	13 22 & semi-diameter.
True Altitude, -	19 15 22

Refraction — Parallax	— 2′ 37″.4	Also — 2 29.7
Semi-diameter,	+ 15 59 5	+ 15 59.5
	+ 13 22.1	+ 13 29.8

39 44
19 52
\+ 13 29.8 Semi-diam. Paral. & Refrac.
20 05 29.8 True Alt. Sun's Centre.

Sun's declin. at noon,	0° 48′ 11″ S.	0° 48′ 11″
3h 46′ 43″ before noon,	+ 3.41	— 3 35.4
Sun's declin. corrected,	0 44 30	0 44 36.6
Cosine, -	9.9999636	9.9999635
Ar. co. cos. Lat.	0.1951520	0.1951520
	0.1951884	0.1951885

Co.-Latitude, -	39° 38′ 45″	39° 38′ 45″
Sun's declination South,	0 44 30	0 44 35.6
Sun's Meridian Altitude,	38 54 15	38 54 09.4
Nat. Sine, - -	627963	627963
Proportional parts for ″	+ 59 Diff. proportional parts, = 23	36
	628022	62799

The 23 Proportional Parts for difference of Sun's declination to be equally distributed from first to last Observation.

Co.-Latitude, -	39° 38 45′										
Sun's declin. S. -	0 44 30										
Sun's Meridian Altitude,	38 54 15										
Nat. Sine, - -	628022....	628020	628017	628015	628013	628010	628008	628006	628004	628001	627999
N. sine 1st Alt. 19° 15′ 22″	629691	331063	332436	333807	335178	336548	337917	339285	340653	342020	343386
Difference, - -	298331	296957	295581	294208	292835	291462	290091	288721	287351	285981	284613
Prop. parts for ″ N. sines,	100	103	107	111	114	118	121	125	128	133	137
Log.	298231	296854	295474	294097	292721	291344	289970	288596	287223	285848	284476
=	9.4745528	9.4725420	9.4705194	9.4684906	9.4664539	9.4644662	9.4623531	9.4602903	9.4582193	9.4561352	9.4540457
Ar. co. Sun's decl. — cos. lat.	0.1951884	0.1951884	0.1951884	0.1951884	0.1951884	0.1951844	0.1951885	0.1951885	0.1951885	0.1951885	0.1951885
	9 6697412	9.6677314	9.6657098	9.6636790	9.6616423	9.6595946	9 6575416	9.6554788	9.6534078	9.6513237	9.6492342
	3174										9839
	572)4238(7 .4										590)2503(4″.2
	4004										2360
	234										143

Time from Noon, -	3 51 17.4	3 50 42 3	3 50 07.1	3 49 31.9	3 48 56.7	3 48 21.3	3 47 46.10	3 47 10.6	3 46 35.2	3 45 59.8	3 45 24.2
Times of Observation,	8 8 42.6	8 09 17.7	8 9 52.9	8 10 28.1	8 11 3 3	8 11 38.7	8 12 14.0	8 12 49.4	8 13 24.8	8 14 00.2	8 14 35.8
Chronometer Times, -	8 1 52.8	8 2 28.2	8 3 3.3	8 3 39.3	8 4 13.4	8 04 48.3	8 5 24.3	8 5 59.3	8 6 36.3	8 7 11.3	8 7 46.3
Chronometer — App. Time,	6 49.8	6 49.5	6 49.6	6 48.8	6 49.9	6 50.4	6 49.7	6 50.1	6 48.5	6 48.9	6 49.5

	6 49.8
	49.5
	49 6
	48.8
	49.9
	50 4
	49.7
	50.1
	48 9
	49.5
	10)6 496.2
Mean, - - -	= 6 49 62
Rate to Noon, -	.61
Difference of Meridians,	— .06
Reduction of Equation to Noon,	+ 3.25
	0 6 52.20
App. Noon, -	11 53 07.80 by Chronometer.

Result of Equal Altitudes observed the same Day with Sextant.

10h 42′ 05″.3	28″.3	11″.3	54″.3	35″.3	20″.3	4″.3	48″ 4	31″.6	18″.3
1 2 55 4	10.8	28.5	47.6	4.3	20.0	33.3	50.6	7.3	20.3
11 45 40.7	39.1	39.8	41.6	39.6	40.3	37.6	39.0	38.9	38.6
11 52 50.3	49.5	49 9	50.8	47.8	50 1	48.8	49.5	49.4	49.8

11 52 50.3
49.5
49.9
50.8
49.8
50.1
48.8
49.5
49.4
49.8
11 52 49.77 Mean

11 52 49.77
\+ 18.03 Equation of Equal Altitudes.
11 53 07.80 Mean of Apparent Noon, by Equal Altitudes.
11 53 07.80 ———— Simple Altitude with Sextant

Sun's Longitude 6s 2° 0′

App. Noon 11h 51′ 41″.7		
Middle Observations,	- -	10h 46′ A. M.
		1 00 P. M.
	Interval,	2 14
	½ Interval,	1 7

XXIII. *Observations on the Junction of the Fresh Water of Rivers with the Salt Water of the Sea.* By the Reverend JOHN FLEMING, D. D. F. R. S. EDIN.

(*Read June* 17. 1816.)

IT is possible, that the following observations may contain little that is new to those who are familiarly acquainted with the details of the science of Hydrostatics. But as I have not met with any remarks on the subject, in the course of my limited reading, the experiments which were performed, and the conclusions to which they lead, are here submitted to the consideration of the Royal Society.

When the flux of the tide obstructs the motion of a river, the wave has been supposed to produce its effects in the same manner as a dam built across a stream. This popular opinion, however, appears to have been adopted without sufficient consideration, as it can only hold true, in those cases, where the opposing fluids are of equal density, but never at the junction of opposite currents of fresh and salt water, which are of different densities. In this last case, where currents of fresh and salt water come in opposition, the lighter fluid, or the fresh water, will be raised upon the surface of the denser fluid, or

 the

the salt water, and when the stronger current of the tide has reversed the direction of the stream, the salt water will be found occupying the bottom of the channel, while the fresh water will be suspended or diffused on the surface. This view of the matter occurred to me in 1811; but it was not until the 29th of September 1813, that I had an opportunity of verifying the conjecture, by an examination of the waters of the Frith of Tay.

Flisk Beach, opposite to which the experiments were made, is situated a considerable way up the Frith, being upwards of sixteen miles from Abertay and Buttonness, where the Frith of Tay actually joins the German Ocean. The channel of the Frith at this place is about two miles in breadth; but upwards of a mile and a half of this extent consists of sand-banks, left dry at every ebb of the tide, and during flood, covered with from three to ten feet of water. These banks are separated from one another by deep pools, or *lakes* as they are termed, which occasion great irregularities in the motion of the currents. The channel of the *river* is near the south side. It is about half-a-mile in breadth, having in the deepest part about eighteen feet of water, when the tide has ebbed, and upwards of thirty feet during flood.

The apparatus which I employed was very simple: It consisted of a common bottle, with a narrow neck, having a weight attached to it. Besides the cord by which the bottle was lowered, there was another connected with the cork, in such a manner, that I could pull it out when the bottle had sunk to the place of its destination. The weather was favourable, and, on the day of the experiment, there was no wind to disturb the surface of the stream.

With this apparatus, I proceeded to the middle of the channel of the river, at *low water*, when the current downwards had ceased

ceased to be perceptible in the boat at anchor; and I obtained water from the bottom, the middle, and the surface of the stream. The water taken from the surface of the stream, was fresh, and tasted like ordinary river water. The water taken from the middle, was not perceptibly different; but that which was brought from the bottom was sensibly brackish. The water from the surface did not contain any salt, as a thousand grains of it, when evaporated with care on a sand-bath, left only a grain and a half of residue, apparently mud, which, when applied to the tongue, communicated no impression of saltness. The water from the middle of the stream yielded two grains of residue, when the same quantity was evaporated, of a whiter colour than the former, and having a perceptibly salt taste. The water from the bottom, which was saltish even to the taste, yielded four grains of saline matter. According to these experiments, the layers of water were arranged according to their densities, the heaviest water occupying the bottom of the stream, and the lightest floating on the surface.

At *half-flood*, I repeated the experiments on the waters obtained from the same situations as before. The water at the surface had now become very sensibly salt to the taste, and thus gave decided proofs of the progress of the tide. The three bottles of water now obtained, yielded results, not in unison with those already taken notice of. The arrangement of the different strata of water, according to their densities, as observed at ebb-tide, was in some degree reversed; for here the water at the surface was salter than that which was obtained from the bottom, and the water from the middle was salter than either. A thousand grains of water from the bottom, yielded by evaporation only ten grains of saline matter, while the water from the surface yielded eleven grains, and from the middle twelve grains, by the same process.

 This

This anomaly is easily accounted for. Were the current of the tide confined entirely to the channel of the *river*, an arrangement of the waters, similar to that which existed in the first experiments, would have prevailed. But during the flowing of the tide, the sea-water soon occupies more than the channel of the river, and spreads itself in various streams among the hollows of the sand-banks. These streams reunite at different places with the principal current, and, in this manner, prevent the salt and fresh waters from gaining their natural relative position. But as soon as these sand-banks are covered with water, the tide proceeds with regularity in its course, so that the different layers of water can then arrange themselves according to their specific gravities.

A thousand grains of water obtained from the bottom, at the *height of flood*, yielded by evaporation twenty-three grains of salt, while the same quantity of water from the middle yielded only eighteen grains; and from the surface only seventeen grains. This was a difference of no less than six grains, and seemed to afford a decisive result.

In order, however, to complete the series of observations, I examined the conditions of the currents at *half-ebb*. The same irregularities prevailed, as before observed at half-flood. A thousand grains of the water, from the bottom, yielded after evaporation eleven grains of salt; from the middle, nine grains, and from the surface, twelve grains. At this time the densest water was at the surface, and the lightest occupied the middle. The cause of this was obvious. Extensive portions of the sand-banks had already been left dry by the receding tide, and various currents of water, disjoined from the main stream by the inequalities of these banks, were now re-uniting with it, through various channels, and disturbing the natural arrangement which had prevailed during the time of flood.

Although

Although the Frith of Tay is very ill calculated for experiments of this kind, from the circumstances already taken notice of, still the premises which we have stated seem to warrant the conclusion, that when the wave of the tide obstructs the motion of a river, and causes it either to become stationary, or to move backwards, the effect is produced by the salt water presenting to the current of the river an inclined plane, the apex of which separates the layer of fresh water from the bed of the channel, and suspends it buoyant on the surface *.

It may here be observed, that this inferior current of salt-water, will never reach that point of the bed of the river, which is intersected by a line drawn perpendicular to the altitude of the wave of the tide, in the ocean, at the mouth of the river. This point is undoubtedly the place at which the salt-water would arrive, at every flood, were there no fresh-water current, as has been demonstrated with regard to the waters of the Tay, by the accurate observations of Mr James Jardine. But as the motion of the current of salt-water is retarded by the opposite current of the fresh-water, and the apex of the wedge which it forms, also washed away by the same agent, the point which the salt-water reaches will be considerably lower than the summit of the tide-wave with which it is connected.

The surface of the higher part of the river, whose elevations and depressions are influenced by the movements of the tide, will necessarily attain a higher level than the summit of the tide-wave, in consequence of the lower specific gravity of the river-water, when compared with the denser column of sea-water,

* I understand that my friend Mr Robert Stevenson has made similar observations at the mouth of the Dee, near Aberdeen, and also on the Thames, and that his conclusions and my own nearly coincide.

which it counterbalances; and this, independent of the progressive motion of the tide in the river.

If the view which we have taken of this subject, in reference to the progress of the salt water, be considered as just, it will enable us to explain some of the phenomena of nature, at present rather perplexing, and may even be useful in its practical application.

In examining the vegetable productions of the banks of rivers, at their junction with the sea, we are sometimes surpried to witness the growth of plants, considered as the natural inhabitants of the sea-shore. But our surprise will cease when we reflect, that the sea-water proceeds farther up the river at every flood-tide than the sensible qualities of the water at the surface indicate; so that the plants, which we hastily conclude to be out of the reach of the salt-water, are still within the sphere of its influence. Thus, at the Beach of Flisk, and even farther up the river, the *Fucus vesiculosus* (the species commonly cut for making kelp) not only vegetates, but in its season appears in fructification.

But that which proves in a still more decisive manner, the action of the inferior stratum of salt-water at the place, is the growth of the coralline termed *Tubularia ramosa* (Ellis's Corallines, Tab. xv. fig. A.), and another of a different genus, closely resembling the *Sertularia gelatinosa* of Pallas. There are likewise some traces of *Flustræ*.

A knowledge of the facts which we have already stated, may be of use to those who are engaged in the erection of salt-works at the mouths of rivers. In such situations, the openings of the pipes for obtaining the salt water, should be placed as near the bottom, or as deep in the water as possible; and water ought only to be drawn during the height of flood-tide, when the fresh-water is diffused over the surface.

Even

Even to navigators, an acquaintance with this subject may sometimes be of use. Thus, for example, when entering a creek in an unknown coast, they may easily ascertain whether any streams of fresh water flow into it, by examining the comparative density of the water taken from the surface and from below.

These experiments appear to give countenance to the opinion which supposes that the water at the surface of the sea contains less salt than the water at the bottom. This may be expected to take place in the neighbourhood of continents, at least, whatever may be the case in the open ocean. During winter, the difference is probably very considerable, as at that season the rivers incessantly pour vast quantities of fresh water into this great reservoir, while but a small portion is abstracted by evaporation. In the Frith of Forth, the difference between the dense water of summer and the diluted water of the winter season, is as eighteen to sixteen, and that even as far down as Prestonpans.

Manse of Flisk,
11*th March* 1816.

XXIV.

XXIV. *Memoir of the Life and Writings of the Honourable* ALEXANDER FRASER TYTLER, *Lord Woodhouselee. By the* Rev. ARCHIBALD ALISON, LL. B. F. R. S. LOND. & ED.

(Read June 3. 1816, and January 6. 1817.)

ALEXANDER TYTLER was born at Edinburgh, 15th October 1747. He was the eldest son of our late venerable associate WILLIAM TYTLER, Esq. of Woodhouselee, in the county of Mid-Lothian, and of ANNE CRAIG, daughter of JAMES CRAIG, Esq. of Costerton, in the same county.

If the most important education is that which is received beneath the paternal roof,—if it is there that the principles and tastes of future life are chiefly formed, the education of Mr TYTLER began under fortunate auspices. His father was a man of high honour, of generous affections, of cultivated taste, and of distinguished eminence in his profession. His mother was a woman of elegant manners, of great gentleness and tenderness of disposition, and of still greater firmness of moral and religious principle. And the society in which they lived was nearly that of all those who then were distinguished in

 this

this city, by their manners, their talents, or their accomplishments. These advantages were not lost upon Mr TYTLER; and in this domestic school he early acquired that taste in life, or that sensibility to whatever is graceful or becoming in conduct or in manners, which ever afterwards distinguished him, and which forms, perhaps, the most important advantage that the young derive from an early acquaintance with good society.

In the year 1755, he was sent by his father to the High School, then under the direction of Mr MATHESON. In that school he remained five years, distinguished to his school-fellows by the gaiety and playfulness of his manners, and to his teachers, by his industry and ability: and, when he left it, he left it with the highest honours which the school can bestow, as Dux of the Rector's, or highest class.

The High School, however, although then a respectable seminary of education, had not yet acquired the eminence which it has since attained, by the zealous activity of the late Dr ADAM, and, more recently, by the enlightened improvements of the present Rector, Mr PILLANS. To complete the classical education of his son, Mr TYTLER, therefore, determined to send him to one of the academies of England; and for this purpose he chose the Academy at Kensington, then under the care of Mr ELPHINSTON, a man of learning and of worth, and distinguished by the friendship of Dr SAMUEL JOHNSON. It was in the year 1763, when he was fifteen years of age, that Mr TYTLER went to Kensington. He was himself at that time conscious of the imperfection of his classical knowledge; he felt that he had yet much to learn, particularly in the articles of Prosody and of Composition, and he entered the academy with the ambition of returning an accomplished scholar. The progress of youth, and the instructions of his father, had now

awakened

awakened him to a sense of the beauties of classical composition; and the names of JOHNSTONE and BUCHANAN reminded him, that the accomplishments which he now travelled to acquire, were once the produce of his own country.

With this ambition, he soon distinguished himself among his school-fellows of the Academy. He became the favourite pupil of Mr ELPHINSTON, and received from that worthy man all that cordial assistance and encouragement which knowledge has so fortunate a pleasure in affording to the ardent and aspiring mind of youth. A little incident, at this time, too, occurred, which served to confirm Mr TYTLER in his love of Latin poetry, and in his ambition to excel in it.

The celebrated Dr JORTIN was at that period vicar of Kensington. Upon some occasion, when Mr TYTLER had particularly gratified Mr ELPHINSTON, by a copy of Latin verses, the good man carried them in exultation to Dr JORTIN. The verses pleased Dr JORTIN so much, that he requested to be made acquainted with the author. Mr TYTLER was accordingly introduced to him. He received him with the greatest kindness, and, after praising the composition, and encouraging his assiduity, he took down a copy of his own Latin poems, and requested Mr TYTLER to accept of it, as a memorial of his approbation and regard. This volume, with a little inscription in the author's handwriting, Mr TYTLER ever afterwards preserved with veneration, and often acknowledged, that much of his attachment to Latin verse was owing to this little incident.

It is among the most important effects of these studies in early life, that they awaken the minds of the young to a new sense of the beauties of Nature, and of the charms of poetical imitation. Both these effects Mr TYTLER seems at this period to have experienced. It was during his residence at Kensing-

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ton, that he first began the art of drawing, and the study of landscape-painting; a pursuit which he continued ever afterwards to follow, and which formed one of the most favourite amusements of his future life. At the same time also, in his hours of leisure, he began by himself the study of the Italian language; and in the early admiration of the poetry of that country, with which his industry was then repaid, opened to himself a field of elegant and of refined amusement, which he never ceased to cultivate with increasing delight.

There was another acquisition which Mr Tytler accidentally made at this time, of which he always spoke with gratitude. It was the love of the science of Natural History. When he went to Kensington, he was particularly recommended by his father to his early friend Dr Russell, the celebrated physician of Aleppo, who at that time resided in the neighbourhood of Kensington; and with this respectable and intelligent man Mr Tytler used always to pass his holidays. Dr Russell was then engaged in the pursuits of natural history; and seeing the ardour of his young friend for knowledge, he made him acquainted with the general principles of the science, associated him as his companion in study, and delighted him, in their leisure hours, by his accounts of the scenery and productions of the East. To these studies Mr Tytler was then alone led by the charm which, in his eyes, they threw over Nature, in the illustrations they every where afforded of the wisdom and benevolence of its Author. He did not foresee that they were afterwards to become to him the source of unfading consolation, and to relieve many an oppressive hour of lassitude and pain.

In 1765 Mr Tytler returned to Edinburgh, after two years passed at Kensington, with equal happiness and improvement. Of these years he always spoke with pleasure, and of

Mr

Mr ELPHINSTON with the most grateful and affectionate regard. He continued ever afterwards occasionally to correspond with him ; and so little did the lapse of time, or the business of mature life, diminish the remembrance of early obligations, that when Mr ELPHINSTON died, he had the satisfaction of associating himself, with his respectable widow, in erecting, in the church-yard of Battersea, a monument to his memory.

In the close of the year 1765, Mr TYTLER entered the University of Edinburgh, and upon a new field of knowledge and of study.

The profession to which his own disposition, and the wishes of his father inclined him, was that of the Law; the profession, of all others connected with literature, most attractive to the ambition of a young man, both by the variety of powers which it demands, and the importance of the distinctions to which it leads. It was to this end, accordingly, that his studies were now chiefly directed ; and although he attended the lectures of Mr RUSSELL upon Natural Philosophy, and of Dr BLACK upon Chemistry, yet he seems to have limited himself to a general knowledge upon the subject of physical science, and to have reserved the vigour of his attention for those classes that more immediately related to his future profession. While he was pursuing, therefore, the study of Civil Law, under the tuition of Dr DICK, and afterwards of Municipal Law, under that of Mr WALLACE, he followed with interest the useful and perspicuous prelections of Dr STEVENSON in the science of Logic: he improved his taste by the celebrated lectures which Dr BLAIR was then delivering upon the subject of Rhetoric and Belles Lettres; and he listened with ardour to that memorable course of Moral Science, in which Dr FERGUSON illustrated, with congenial power, the various systems of ancient

philosophy,

philosophy, and occasionally exhibited all the splendors of ancient eloquence.

Of the progress or success of Mr TYTLER's studies during these years, no record, indeed, remains in the annals of the University. It has been the practice, and perhaps the wisdom of the Professors of that distinguished Seminary, to seek more to gratify the desire of knowledge in the young, by the instruction they convey, than to stimulate it by the distinctions they confer; and to look for their reward rather in the future eminence of those they instruct, than in the display of early and premature exertion. Of the dispositions or attainments of the young, however, there is, at this age, one unfading proof to be found, in the character of the friends and associates whom they select. The circumstances of the times, and the celebrity of the Professors, had at this period excited in the young men of the University, an unusual spirit of literary ambition, and many of those who have now arisen to the highest distinctions in their country, were at this time laying the foundations of the eminence to which they have attained. It was in this class that Mr TYTLER sought for friends, and it was in this class he found them. The vivacity of his temper, the variety of his attainments, and the high spirit of honour which distinguished even his earliest years, rendered him acceptable to all the young and spirited of his own age; while his zeal for knowledge, and his ambition of distinction, conciliated the regard of those who were older. It was in these years, accordingly, that the great friendships of his life were formed; and it was his peculiar happiness, that among those to whom the affections of his youth were given, the course of his mature life was passed, and its final period was closed. The list is an ample one, and will not be heard in this Society without emotion; for it contains the names of HENRY MACKENZIE, of ALEXANDER

ABERCROMBIE

ABERCROMBIE (late Lord ABERCROMBIE), of WILLIAM CRAIG (late Lord CRAIG), of ALLAN MACONOCHIE (late Lord MEADOWBANK), of WILLIAM ADAM (now Lord Chief-Commissioner), of ROBERT LISTON, of ANDREW DALZEL, of WILLIAM ROBERTSON (now Lord ROBERTSON), of JOHN PLAYFAIR, of Dr GREGORY, and of DUGALD STEWART,—men, whom in this place it would ill become me to insult with praise, but from whose friendship, I may be permitted to say, there is no name so illustrious that would not derive distinction.

If the seasons of academical study were thus happily and usefully employed by Mr TYTLER, the seasons of the summer vacation were not less so. Upon these occasions, he retired to Woodhouselee, the beautiful seat of his father, near Edinburgh, a scene endeared to him by the remembrances of infancy,—by all the ties of domestic affection,—by the improvements which his father was then annually adding to it,—and, perhaps, by those anticipations of greater embellishment which it was afterwards to receive from his own hands. Amid the solitude and quiet of this romantic residence, and at a distance from the prescribed routine of academical labour, he felt all the happiness that arises from the freedom of study, and was at liberty to follow out, without interruption, those literary pursuits to which inclination and taste most strongly inclined him. The character of his age, and of his mind, led him naturally to those compositions which, as addressed to the imagination and the heart, constitute the polite literature of every country. His knowledge, both of the ancient and the modern languages, enabled him to indulge this desire; and in the course of successive summers, he seems to have formed and to have executed, with this view, a plan both of comprehensive and of systematic study.

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He began with the great writers of antiquity,—the Poets, the Orators, and the Historians of Greece and Rome, to whose works he now returned with that increase of knowledge, and that improvement of taste, which enabled him more fully to seize and to appretiate their various excellence. He next resumed, (though with more enlightened views), the study of Italian literature, and perused with new admiration the writers of that brilliant period which succeeded the revival of letters in Europe, and who, though formed, in the great principles of composition, upon the models of classic taste, have yet added to them all the splendid courtesy of feudal manners, and all the romantic interest of chivalrous adventure. After the extinction, or (as I trust) only the slumber of Italian genius, he followed the progress of taste into France, and pursued the singular history of composition in that country, from the period that the genius of CORNEILLE first gave to its imperfect language the dignity of poetry, to the time that the eloquence of FENELON, of BUFFON, and of ROUSSEAU, rose above the level of its poetic diction, and gave to prose composition all the powers and all the pathos of poetry.

The study of foreign literature led Mr TYTLER naturally to that of his own country, and, in comparing the great writers of England with those of the different nations of the Continent, he was enabled to form a more accurate estimate, both of the extent of English genius, and the powers of the English language. While engaged in this pursuit, his curiosity was led into a field at that time little cultivated in this country, I mean to the study of the ancient writers of England, those original masters of composition, in whose writings the genius of the people and of the language is most strongly displayed, and who conducted him (in the language of SPENSER) to " *the pure well of " English undefiled.*" The pursuit not only rewarded him at the

the time, but tended to form his taste in future days; and he was among the first literary men of this country, who felt the beauty of our language in its first stage of improvement, and foresaw the advantages that the study of our earlier writers would give to modern composition, by introducing greater unity of character, a purer analogy of construction, and the peculiar energy that arises from idiomatic expression.

The same taste which guided the studies of Mr TYTLER at this period, directed also his amusements. The art of Drawing, which he had at first begun to practise at Kensington, he now resumed with ardor, amid the beautiful scenery he inhabited. The love of Music, which was hereditary in his family, had been cultivated by the example, and under the instructions of his father, and he willingly became a performer, not only to indulge his own taste, but that he might add his assistance to the little family concerts with which that excellent man loved always to close his active day. But the amusement in which at this period Mr TYTLER peculiarly delighted, was that of making excursions to visit the remarkable scenery, either of England, or of his own country. He had an early love of the great and beautiful in Nature; and his sensibility in this respect had been increased by his study of landscape painting. But his taste was not of that servile kind, which looks only to the art of imitation: and he felt that there were many other sources of beauty in the scenery of Nature than the painter can employ. His mind was open, not only to all those moral expressions which form what has been called the *poetry of Nature*, but to all those local and accidental expressions which it receives from the events of time; and he loved to mingle in such scenes, with the sensibilities of taste, the associations of poetical description, and the memory of historical events. In this manner, Mr TYTLER used always to pass some parts of the

 summer

summer or autumnal months; and, in the course of a few years, there were few scenes, either in England or in Scotland, which he had not visited, that were distinguished, either by natural beauty,—by poetical celebration,—by the residence of eminent men,—or by the occurrence of memorable transactions. In such employments, to him (as to all who are capable of it) there was something more than amusement; and he never returned from them, without feeling his taste improved, his ardour in study animated by the memories of illustrious men, and his love of his country increased, both by the monuments of its former glory, and the appearances of its progressive prosperity.

In the year 1770, Mr Tytler was called to the Bar; and in the spring of the succeeding year, he accompanied his friend and relation Mr Kerr of Blackshiels on a tour to Paris, from which they returned by Flanders and Holland.

The year 1776 was marked by the most important as well as the most fortunate event of his life, by his marriage to Miss Anne Fraser, eldest daughter of William Fraser, Esq. of Balnain,—an union which had long been the object of his secret wishes,—which now accomplished all the hopes he had formed of domestic happiness,—and which, after the long period of thirty-six years, unclouded almost by misfortune or distress, closed at last in more grateful and profound affection than it at first began.

At this period, when the business and the duties of life were opening fully upon him, Mr Tytler seems to have made a very deliberate estimate of the happiness that was suited to his character, and to have marked out to himself, with a very firm hand, the course he was afterwards to pursue. His profession opened the road both to professional fame, and to civil distinction, and the circumstances of the times were of a kind to animate

mate all his ambition of literary distinction. The period to which I allude, was perhaps, indeed, the most remarkable that has occurred in the literary history of Scotland. The causes which, since the era of the Union, had tended to repress the spirit of literature in this country, had now ceased to operate: the great field of England was now opening to the ambition of the learned; and the ardour with which they advanced into it, instead of being chilled by national prejudice or jealousy, was hailed by the applause of that generous people. The fame of Mr Hume was now at its summit of celebrity. After the honours with which the *Histories* of *Mary* and *Charles* V. were crowned, Dr Robertson was laying the foundation of new claims to historical reputation; and in the solitude of his native village, Mr Smith was preparing that illustrious work which was afterwards to direct the laws, and to regulate the welfare of nations. The different Universities of the country were vying with each other in the ardour of scientific pursuit, and in the dissemination of useful knowledge; and from them there were annually advancing into life, some of those men who have since supported or extended the reputation of their country. The profession of law partook in the general spirit of improvement: the pleadings of the Bar began to display a more cultivated taste, and the decisions of the Bench to be directed by a more enlightened philosophy. The eloquence of Mr Lockhart was still occasionally heard; and Mr Erskine was beginning that brilliant career which so lately only has been closed. Lord Hailes was carrying into the obscurity of our antiquities the torch of severe but sagacious criticism; and Lord Kames was throwing over every subject almost of science or of literature, the lights of his own original and comprehensive genius.

These were circumstances sufficient to excite and to justify ambition; but although Mr TYTLER was ambitious, it was not so much of fame he was ambitious, as of usefulness. The modesty, as well as the benevolence of his nature, disqualified him for those adventurous speculations, in which nothing but personal celebrity is attained; and in looking at the literary scene before him, the path that invited him, was not that which rises amid dangers and difficulties into solitary eminence, but that which follows out its humbler and happier way amid the duties and charities of social life. In all his ambition, too, there was (if I may use the expression) something always domestic. The honours to which he aspired were those which he could share with those he loved, and the " eyes" in which he wished *to read his history*, were not so much the eyes of the world, as those of his family and friends. It was with this moral and chastised taste that he looked even to the honours of his profession: And when he recollected the brightest distinction it ever received, it was not CICERO in the Forum or in the Senate House, that was so much the object of his admiration, as CICERO at his Formian or his Tusculan Villa, amid the enjoyments of domestic friendship, and the delights of philosophic study.

With these dispositions, Mr TYTLER soon found, that the share of business which a young man can acquire at the Bar, was insufficient to employ the activity of his mind, and that the merely occasional attention which particular cases required, was at variance with those habits of continued study in which he was accustomed to be employed. To consider law as a science was more congenial to his mind, than to consider it only as a profession; and he became desirous, therefore, of engaging in some continued work, where (like some eminent men before him) he might entitle himself to the honours of his

his profession, rather by the labour of solitary study than by the celebrity of actual practice. While he was forming this resolution, the advice of his patron and friend Lord KAMES, not only encouraged him to execute it, but suggested to him also a subject in which it might usefully be executed. As this incident gave origin to the first work which Mr TYTLER published, and as it is descriptive of the benevolent attention of that distinguished man to his younger friends, I am happy to be able to relate it, in Mr TYTLER's own words, from a little manuscript account of the principal events of his life, which he has left for the instruction of his family.

——" The first time (says he) I became intimately acquainted with Lord KAMES, was, I think, in autumn 1767, when " he asked my father and me to accompany him on the Southern Circuit. We passed a few days with him at his estate of " Kames, and thence travelled to Jedburgh and Dumfries. " From that time I had the satisfaction of perceiving that I " had some share in his good opinion, of which he gave me " many proofs. While prosecuting my studies in the law, I " was wont frequently to resort to him for his advice, and in " the vacations I made many excursions to Blair-Drummond, " where I staid for ten days or a fortnight at a time, and partook in all his occupations either of study or of amusement. " Having read to him a little literary Dialogue which I had " composed, with which he was pleased, he gave me his advice, to fill up my intervals of leisure by composing a set of " Literary Essays: In consequence of which, I wrote a few detached sketches, which I shewed him from time to time. It " was upon one of these visits to Blair-Drummond, about three " years after I had put on the Gown, that, in talking of some " of his Law Works, he asked me if ever I had attempted to " write any thing in the way of my profession. I told him

" that

" that I had not, but that I was at that time meditating some-
" thing of that kind. He then proposed to me to write a Sup-
" plemental Volume to his Dictionary of Decisions, bringing
" down that Work to the present time. I told him, that the
" boldness of the undertaking terrified me; but that the good
" opinion he had shewn of me by making such a proposal, was
" certainly a strong inducement to me to make the attempt.
" I took, however, some time to deliberate upon it; and ha-
" ving at length resolved to undertake the work, I went out
" again to Blair-Drummond, to inform myself of the method
" he had followed in abridging and arranging the cases. These
" he communicated to me, and I set to work under his eye.
" The simple abbreviation of the printed cases occupied me
" above four years, and during all that time I read over occa-
" sionally to Lord KAMES, the sheets of my abstracts, on which
" he gave me his notes and emendations. The arrangement
" of the cases gave me another year's employment; and while
" this was going on, I shewed the sheets, from time to time, to
" Lord KAMES, a great part of them to Mr ILAY CAMPBELL,
" as also to the Lord President DUNDAS, to all of whom I was
" much indebted. When the Work was completed and print-
" ed, I was much gratified to find that Lord KAMES was plea-
" sed with it. Some passages in the Preface, apologizing for
" defects, he desired that I would strike out. 'The *Work*,
" (said he,) *does you honour; and a man ought not too much to*
" *undervalue his labour, or depreciate his own abilities*." This
volume of the Dictionary of Decisions was published in folio,
in 1778; and of the character and value of the work, no other
testimony is necessary after the sanction of the great Lawyers
that have been mentioned.

Mr TYTLER had now avowedly dedicated his life to the pursuits of Literature, and his friends became anxious to see him placed

placed in some one of those public literary stations, where his talents and his industry might be more conspicuously displayed than in the retirement of private study. An opening of this kind soon occurred, which Mr TYTLER willingly embraced. The late JOHN PRINGLE, Esq. had been recently appointed to the Professorship of Universal History and Roman Antiquities in the University of Edinburgh; but finding the discharge of the duties of it incompatible with his other employments, had expressed his inclination to resign it. The Class, (I believe,) in its original institution, in this and in other Universities of Scotland, had been intended as subsidiary to the study of the Civil Law. It had been taught always by Members of the Faculty of Advocates, and attended by students of that description: And it had, therefore, that degree of relation to Mr TYTLER's own profession, that forfeited none of the hopes or expectations he might form of its future distinctions. An arrangement was soon made with Mr PRINGLE. In 1780, Mr TYTLER was appointed Conjunct Professor, and in 1786, sole Professor of Universal History.

From that period until the year 1800, Mr TYTLER devoted his life almost exclusively to the duties of his Professorship; and ten years of assiduous study were employed in the composition and improvement of the Course of Lectures which he annually read in the University.

Of the character and value of that Course of Lectures I should have felt it a duty to have attempted some slight description, if I were not prevented by the presence of many, to whom every attempt of this kind would be superfluous, and by the recollection, that while they remain unpublished, they cannot be the objects of public criticism. I may be permitted, however, to offer to the Society a few observations upon the views with which Mr TYTLER entered upon his Professor-

ship,

ship, and upon the plan he pursued in the conduct of his Lectures.

The Class had hitherto been taught chiefly in relation to the Science of Law, to which it was considered as subsidiary. It was not so much Universal History that was the subject of prelection, as the History of Rome; and the views that were exhibited of Roman Antiquities, were chiefly those that were illustrative of the principles or progress of the Civil Law. Mr Tytler felt that it became him to take a more comprehensive view of the subject; to aim at higher utilities than those of a single profession; to adapt his Lectures to the more liberal opinions which had arisen with regard to education, and the increasing celebrity of the University where they were to be delivered; and *in the course of them*, (as he has himself expressed it,) *to exhibit a progressive view of the state of mankind from the earliest ages, of which we have any account,—to delineate the origin of states and empires,—the great outlines of their history,—the revolutions which they have undergone,—and the causes which have contributed to their rise and grandeur, or operated to their decline and extinction.*

In the execution of a design so extensive, Mr Tytler's attention was first directed to the choice of a *Plan*, or to the formation of a system of arrangement, by which he might be able to give some degree of unity and consistence to the great mass of materials that were before him. In examining the methods in which Academical Lectures on this subject had hitherto been conducted, either in this country or on the Continent, he perceived that there were two different systems which had chiefly been followed, and which may, perhaps, not improperly be styled the Narrative, and the Didactic Systems. In the first, the principle of arrangement was simply that of Chronology: the only order observed was the order of time, and the

the only object of the teacher was to convey to the student the knowledge of the succession of historical facts. In the second, the principle of chronological arrangement was altogether disregarded: the events of history were considered not as a branch of knowledge in themselves, but as a ground work for the conclusions of science; and the great object of the teacher was to convey to the students the knowledge of the general principles of public law and of political philosophy.

In neither of these systems did Mr TYTLER find the utilities which it was his ambition to derive from the subject of his lectures. The first appeared to him only a barren detail of chronological events, in which nothing more was conveyed than the mere knowledge of the succession of these events; and all that is included under the name of the Philosophy of History was necessarily omitted. In the second, he feared that too wide a field was opened to the ambitious speculations of the teacher, and that while the attention of the student was liable to be occupied by hasty or by unfounded theories, the interest of historical narration was necessarily lost, and all the moral instructions of history neglected.

The system which Mr TYTLER finally adopted for his own course of lectures, was one which combined the advantages of both these systems, and was very happily adapted, both to maintain the interest and to consult the instruction of the student. In surveying with an attentive eye, the ancient history of the world, he observed, (to use his own words,) that it was distinguished, in every age, by one prominent feature; *That one nation or empire was successively predominant, to whom all the rest bore as it were an under-part, and to whose history, we find that the principal events in the annals of other nations may be referred from some natural connection.* In this remarkable

feature Mr TYTLER saw that a principle of natural arrangement was afforded him, which might give to his course a sufficient degree of unity and order; and which, while it preserved to the student the interest of historical narration, gave to the teacher the opportunity of exhibiting those general views of the progress of the human race, which form the most important instruction we can derive from its history.

It was on this principle that his course of Ancient History was conducted. After some general prospects of what is known of the Assyrian and Egyptian Empires, he began with the brilliant and interesting subject of Greece. He treated at length, the events of its civil and political history, and in conducting his narrative, brought occasionally into view the situation of the nations by which it was surrounded. He then examined the nature of the various governments which distinguished it,—the different political institutions which they had adopted,—the character of their military establishments,—their principles of colonization, and of internal regulation: And when time had conducted him to the melancholy period of the extinction of their independence, he took a retrospective view of its literary history,—of the state of its attainments in arts and science, and, above all, of the nature and causes of that unequalled excellence which it attained in all the arts of taste.

The next great subject which presented itself was the history of Rome: and in the views he took of this magnificent portion of his course, he followed the same arrangement, and employed the same method of instruction. After recounting its obscure origin and infant institutions,—after tracing the progress of its political constitution, until it terminated in that illustrious Republic, which, though so long extinct, still reigns, as by some magic spell, over the minds and imaginations of mankind,

mankind,—he followed the progress of its arms through a world hitherto unknown; and thus gradually introducing to the observation of his students, those various nations of the North, that were destined in future years to overturn this mighty fabric, he made the easiest, but the most fortunate, transition to the history of Modern Europe, and to the examination of the causes that produced the fall of Rome. At this eventful period, he again availed himself of the pause which history afforded him, to take a retrospective view of this great people,—to consider their attainments in arts and arms,—to compare their progress in science and in literature with that of the mighty people that had preceded them,—and to indulge himself in that illustration of the excellence of their greater writers, which he was so well qualified to give, and which, far better than mere critical examination was fitted to excite the admiration, and to form the taste, of the young who heard him.

The history of Modern Europe afforded not to Mr Tytler the same fortunate principle of arrangement which he had found in the Ancient: But another principle of connection presented itself, of which he willingly availed himself. To the historian of Modern Europe, the natural place of observation is his own country. It is the point of view to which all his interests most obviously conduct him, and from which all the events of the surrounding world fall into somewhat of systematic order and harmonious distance. It was on this principle, therefore, that Mr Tytler conducted his views of modern history. Considering the history of their own country as the subject most important in the instruction of his students, he began by the narration of the great events of its civil and military story: He traced the successive steps of its progress in industry, in legislation, in opulence, and in refinement; and unfolded with care the gradual rise of its political

constitution, until it terminated in the memorable era of the Revolution. From this central point of observation, he took occasion, at different times, to direct the attention of his students to the contemporary history of mankind,—to mark to them the successive changes that were occurring upon the Continent of Europe,—to introduce to them those new empires which at one period the frenzy of fanaticism, and at another the avarice of commerce, had revealed to the European eye,—and to awaken their attention to the mighty consequences which the establishment of Christianity, the invention of printing, the discovery of the New World, and the spirit of the Reformation, have had upon the general character and manners and happiness of modern times. With these great subjects he gladly at times interwove the history of literature and science; and while his attention was chiefly directed to the progress of British literature, he led the observation of his students to the contemporary history of learning upon the European Continent, and to the examination of those general causes which had influenced the successive steps of its progress, from the time of the revival of letters to the brilliant period when his lectures closed.

The success of this course of lectures was sufficient, (as Mr Tytler has said,) " to compensate the labours of the author." They came to form an important part in the system of general education; and he soon numbered among his students, not only those who were destined to the profession of the law, but the young of every different description, whose education was conducted upon liberal and philosophical principles. The little volume which he published in 1782, under the title of *Outlines of a Course of Lectures*, for the assistance of his students, became so popular, that he found himself called upon to present it to the world, in a larger form, under the title of *Elements of General*

neral History, in two volumes. This work has since passed through four editions, and has been found so useful by those engaged either in the business of private or public education, as affording a concise and luminous arrangement of historical events, that it is now used as a text-book in some of the principal seminaries of education in England, and has become (as I understand) the ground-work of historical study in some of the Universities of America.—Of the lectures themselves, while they remain unpublished, it would be preposterous to offer any opinion: yet, when they are given to the world, I shall be much deceived, if they are not found to fill up an important desideratum in English literature,—to afford to the minds of the young more pleasing and more enlightened views of the history of Man, and the progress of the Human Race, than any other similar work in our language presents them, and to accomplish the generous ambition of their author, *in rendering the study of history subservient to the great end of all education, that of forming good men and good citizens.*

The labours in which Mr TYTLER was thus employed, were sufficient to occupy, but not to engross, his attention. He continued assiduously his practice at the Bar; and he followed, with the interest of a man of letters, the progress of Science and Philosophy around him. The reputation which his taste and talents had now acquired, created many appeals to him for literary advice or assistance, and to him every labour was welcome, in which he could serve the cause either of literature or of friendship.

In 1778, when Dr GREGORY was publishing an edition of the Works of his Father, Dr JOHN GREGORY, he solicited Mr TYTLER to prefix to it a short account of his life and writings. It was a task which Mr TYTLER willingly undertook, from his early

early acquaintance with that eminent and amiable man, and he executed it with the simplicity almost of filial reverence and affection.

The year 1779 was distinguished in this country by the appearance of the celebrated periodical paper, *The Mirror*. Of the progress of a work which, both in its design and execution, did so much honour to Scotland, Mr TYTLER could not be an indifferent spectator. Although not properly a member of the Society, he was yet the friend of all who were known to be members of it. To the beauty and excellence of the serious papers in this work, Mr TYTLER felt that nothing could be added; but it seemed to him that something was wanting upon the side of levity and gaiety; not only for the purpose of temporary popularity, but to give to the serious papers themselves their proper importance and relief. With this view, he contributed to the Mirror the papers, Nos. 17. 37. 59. and 79.; and in 1785, to the Lounger, the papers, No. 7. 9. 24. 44. 67. 70. 79.

Of these papers the original manuscript happens still to remain; and it affords a very pleasing memorial of the manner in which Mr TYTLER was accustomed to pass his most vacant hours. The manuscript occupies the blank leaves of some sketch-books with which Mr TYTLER always travelled, for the purpose of landscape-drawing, and was written at inns, in the evenings after the journeys of the day were done. It was in this manner that the chearful activity of his mind found employment and amusement every where; and that the hours which most men pass in indolence or fretfulness, were passed happily by him, in the offices of friendship, or in the enjoyments of elegant composition.

On the institution of the Royal Society in the year 1783, Mr TYTLER was one of its constituent members, and was unanimously

animously elected one of the Secretaries of the Literary Class,—an office which he continued to fill with zeal for many years; and in the execution of which he drew up that "*Account* "*of its Origin and History*," which is prefixed to the 1st volume of its Transactions.

In 1788, he contributed to the Royal Society a biographical Memoir of the late ROBERT DUNDAS of Arniston, Lord President of the Court of Session,—a paper valuable, not only for the just and vigorous delineation which it gives of the character of that eminent Judge, but for the interesting account it affords of some of the earlier branches of a family, so long and so honourably distinguished in the legal annals of Scotland.

In 1789, Mr TYTLER read a paper to the Royal Society upon *the Vitrified Forts in the Highlands of Scotland.* Of these singular antiquities, the prevailing theory had been, that the vitrification was produced in the process of their erection, and that it was the substitute of a rude age for cement. The theory which Mr TYTLER suggested was the reverse of this;—that the vitrification was the result,—not of their erection, but of their destruction,—and that it was produced by the efforts of enemies in attempting this destruction by fire. The theory is certainly not without some appearances of probability: it assimilates sufficiently with the period of society to which such buildings undoubtedly refer; and Mr TYTLER was able to support it with learning and ingenuity. Of the impression it made at the time upon the Society, I am happy to be able to refer to an evidence of no little weight, in a letter from our late illustrious associate Mr SMITH to Mr TYTLER upon the subject; and, although the letter is very short, I persuade myself that it will not be unacceptable to the Society, both because there are unhappily very few letters of this great man remaining, and because it involves also the memory of some other men,

whose

whose names can never be listened to in this place without emotion:

"Dear Sir,

"I have read over your paper with the greatest pleasure. The composition is what it ought to be, simple, elegant, and perfectly perspicuous, and will be a very great ornament to our Memoirs. Some of my chemical friends, however, are of opinion, that the degree of vitrification which takes place in the specimens of these forts, is too great to be the effect of any accidental fire, such as you suppose, and could be produced only by a great accumulation of wood, heaped upon the wall after it was built. This is a subject of which I am ignorant. You had convinced *me*, who fancied that this imperfect vitrification was more likely to be the effect of accident than of knowledge. The friends I mean, are Dr Black and Dr Hutton, who in every other respect entertain the same high opinion of your composition which I do. You had better converse with them: you may convince them, or they may convince you; and even though neither of these two events should happen, the offence, I apprehend, will not be great, either to them or to you. I have the honour to be, &c.

"Adam Smith."

In the year 1790, Mr Tytler read in the Society those papers on Translation, which they who heard them will remember to have been listened to with so much pleasure, and which he soon after published without his name, and under the modest title of an *Essay on the Principles of Translation.* The work was scarcely published, when it occasioned a correspondence with the late learned and ingenious Dr Campbell, Principal

cipal of Marischal College, Aberdeen, from which, however painful at first, Mr TYTLER might easily have foretold its future fortune in the literary world. Dr CAMPBELL had, some time previous to this, published his Translation of the Gospels, to which he had prefixed, in a preliminary dissertation, some very acute and ingenious observations upon the principles of translation. Upon the publication of Mr TYTLER's anonymous work, he immediately procured it, and was so much struck with the coincidence of their views upon the subject, that he wrote to his printer Mr CREECH, to know who was the author; and while he acknowledged himself "*to have been flattered not a*" "*little to think, that he had in these points the concurrence in judg-*" "*ment of a writer so ingenious,*" he expressed at the same time some suspicion, that the author might have borrowed from his Dissertation, without acknowledging the obligation. Mr CREECH, with great propriety, communicated the letter to Mr TYTLER; and he instantly wrote to Dr CAMPBELL, acknowledging himself to be the author, but assuring him, that the coincidence of sentiment was purely accidental, and that the name of Dr CAMPBELL's work had never reached him until his own had been composed. "*The coincidence of our general principles,* (says" "Mr TYTLER), *is indeed a thing flattering to myself; but I can-*" "*not consider it as a thing at all extraordinary. There seems*" "*to me no wonder, that two persons, moderately conversant in*" "*critical occupations, (I am far from thinking equally so), sitting*" "*down professedly to investigate the principles of this art, should*" "*hit upon the same principles, when in fact there are none other*" "*to hit upon, and the truth of these is acknowledged at their first*" "*enunciation. In my opinion, there would, on the contrary, be just*" "*matter of wonder if they did not hit upon them. But in truth,*" "(concludes Mr TYTLER), *the merit of this little essay, (if it has*" "*any), does not, in my opinion, lie in these particulars. It lies in the*

" *establishment*

" *establishment of those various subordinate rules and precepts, which* " *apply to the nicer parts and difficulties of the art of translation;* " *in deducing those rules and precepts which carry not their own* " *authority* in gremio, *from the general principles which are of* " *acknowledged truth, and in proving and illustrating them by* " *examples. How far you may have anticipated me even in this* " *respect, I cannot say, until I have perused your Dissertations.* " *They appear to contain a rich mine of philological and critical* " *learning; and I am confident, that if my book comes to a se-* " *cond edition, I may be able to profit much by your remarks. In* " *that case, I shall most cordially, and with the highest pleasure,* " *acknowledge my obligations.*"

To those that are acquainted with the character of Dr Campbell, it will be unnecessary to add, that he received Mr Tytler's explanation with the most candid and polite liberality. " *The letter you favoured me with,* (says he), *made me both* " *ashamed and vexed, that I should have been so rash as to ex-* " *press myself to* Mr Creech *in a manner which could give a* " *moment's uneasiness to a man of merit, especially one whom I* " *consider myself as having the honour to call a friend. When* " *I wrote that letter, I neither knew nor suspected who the au-* " *thor of the Essay was. Had I known what I now know, the* " *name of the author alone would have convinced me that the co-* " *incidence was merely accidental.—Your arguments are good,* " *but I was sorry you had recourse to them; sensible as I am,* " *that if your declaration had not been sufficient to satisfy me, I* " *did not deserve to be satisfied. Mathematical demonstration,* " *were you to attempt it, would not give me stronger conviction* " *than I already have, that what you say is the truth.——But to* " *have done with the disagreeable part of this mistake,* (he concludes), *I cannot avoid mentioning one circumstance in this in-* " *cident, which to me is always extremely agreeable, the evidence* " *which*

"*which it gives of a concurrence in sentiment upon critical sub-*" "*jects with persons of distinguished ingenuity and erudition.*" "*Such a discovery makes a man more confident in the justness of*" "*his own sentiments. I have only to add, that your illustrations*" "*of the general doctrines, and your examples from the ancients,*" "*please me exceedingly.*"

The opinion of Dr CAMPBELL was very soon justified by the voice of the literary world; and I believe that there is no work of literary criticism which this country has produced, that so soon attained celebrity in England, as the Essay on Translation. The different reviewers of the day, contended with each other in the earliness of their notice, and in the liberality of their praise. The most celebrated scholars of England, Dr MARKHAM, Archbishop of York, Dr DOUGLAS, Bishop of Salisbury, Dr PERCY, Bishop of Dromore, Dr VINCENT, of Westminster, and Dr WATSON of Winchester Schools, wrote to the author in terms of high approbation. "Were I not afraid," says Mr MURPHY, the well-known translator of *Tacitus*, in a letter to the author, "*of being thought a dealer in compliment, I should say, that I esteem*" "*it the best performance I have ever seen on the subject. In-*" "*genious hints, and cursory remarks, are to be found in many*" "*authors, ancient and modern; but they remained scattered, and*" "*nothing like a regular system has been formed until now.*" And Mr CUMBERLAND, the extent of whose learning, and the fertility of whose genius gave so much value to his opinion, was so much delighted with the work, and so grateful for the just praise which Mr TYTLER had bestowed upon his admirable translations from the fragments of Greek comedy, that he wrote to his friend Sir WILLIAM FORBES, to beg of him to procure Mr TYTLER's permission to dedicate to him a translation of *The Clouds of* ARISTOPHANES, which he was then preparing, and which the praise of so distinguished a critic had encouraged

 him

him at first to undertake. To the opinion of these eminent men, it may be supposed I very willingly subscribe; yet, I must add, that the work has always appeared to me as entitled even to a higher praise. In its plan, indeed, it appears to relate only to the principles of translation; but in its execution, it necessarily involves the principles of composition in general; and in the nature and variety of the examples he adduces, and the acuteness and delicacy of the criticism he employs, Mr TYTLER seems to me to have made use of one of the happiest methods to lead the minds of his readers to a sense of those fine and evanescent beauties in composition, which abstract language can so imperfectly express, and which affords the best preparation, not only for the task of translation, but for the higher purpose of original composition.

The *Essay on the Principles of Translation* has now passed through five editions, in each of which the author has been anxious to repay the approbation of the public, by the additions he has made; and after the experience of fifteen years, it may now be considered as one of the standard works of English criticism.

While Mr TYTLER was thus actively and usefully employed, the Government of Scotland began to consider him as one who was fitted to share in its administration, and Lord MELVILLE thought himself now entitled, by the character which Mr TYTLER had established, to testify to the public the sentiments of his private friendship. His practice at the Bar, though not extensive, had been respectable, and, in the conduct of it, he had shewn sufficiently the talents he possessed for business. His honour was high,—his integrity acknowledged,—and his manners amiable and conciliating. His political opinions were those of hereditary loyalty; and in the acceptance therefore of office, he had none of those sacrifices of principle to make, by which

which the course of political ambition has been sometimes degraded. In the year 1790, he was appointed Judge-Advocate of Scotland, in the room of Mr Charles Hope.

The office of Judge-Advocate, it had hitherto, (I believe), been usual to execute by deputy; but Mr Tytler was not of a character to make any compromise with duty, or to accept of office, without accepting of all its obligations. He made it his business, therefore, to attend upon every trial: he gave to every case his most careful and considerate attention; and so anxious was he to fulfil his duty to the utmost, that he took the trouble of drawing up, for his own direction, a *Treatise upon Martial Law*, which afterwards, when he retired from the office, he gave to the public, and which has (I understand) been found of the most important use in the decision of cases of this kind.

Into the detail of Mr Tytler's conduct in the discharge of this delicate but important office, it would be presumptuous in me to enter; but I may be permitted to relate, from his correspondence, a single incident, which illustrates both the consciousness with which he discharged his duty, and the respect in which his opinion was held by those who were then at the head of the Military Department.

A court-martial had been held at Ayr, with the sentence of which Mr Tytler was extremely dissatisfied, and to the injustice of which, he had anxiously, but in vain, endeavoured at the time to awaken the attention of the Court. Upon transmitting the proceedings to London, Mr Tytler thought it his duty to communicate the grounds of his dissatisfaction with the sentence to Sir Charles Morgan, then Judge-Advocate-General, and, in the most earnest terms, to implore his attention to the case, if his Majesty should (as was probable) refer it to his decision. Sir Charles Morgan cheerfully undertook the

the revision of the case: his opinion coincided in every respect with that of Mr Tytler; and to the letter in which Sir Charles communicated to him his Majesty's disapprobation of the sentence, Mr Tytler added the following note: "I have thus "had the satisfaction of procuring from his Majesty a disap-"proval of this very unjust sentence, and a rectification of it "in every point where it was wrong."

In the year 1792, Mr Tytler had the misfortune to lose his father, at the advanced age of eighty-one. Of the character of this excellent man, the Society already possesses a description by Mr Mackenzie, which no one will attempt to improve. The loss to his son was of a kind which it is the fortune of few men to experience. Their connection had subsisted for the long period of forty-five years, undiminished by distance, and unbroken by misunderstanding; and there was so singular a correspondence in their tastes, their pursuits, their principles, and even their prejudices, that Mr Tytler felt he had not only lost a father, but his best and oldest friend. His first employment was to design a little monument to his memory, which he soon after erected in the pleasure-grounds of Woodhouselee, upon a spot which his father had particularly loved; and he engraved upon it the following inscription, which so well expresses the filial tenderness of the author, and so happily obeys that profound and merciful propensity of sorrow, which leads us still to fill the scenes we love, with the presence of those we have lost.

M.

M. S.

GULIELMI TYTLER, de Woodhouselee,

H. L. P. F.

En virides aras, en hanc quam ponimus urnam,
Tu, fili ex manibus respice dona, Pater!
Sic, venerande Senex, olim qua rura placebant
Sint eadem busto nunc decorata tuo.
Neve Tibi desit post funera sueta voluptas,
Proximo ab umbroso cantet avis nemore,
Et qui Te placido lenibat murmure rivus,
Dulcia perpetuis somnia portet aquis.

By the death of his father, Mr Tytler had succeeded to the estate of Woodhouselee; and some years before that period, Mrs Tytler had, in a similar manner, succeeded to the paternal estate of Balnain in Inverness-shire. He was now in circumstances of affluence,—his friends were numerous,—his own disposition in the highest degree hospitable and kind,—and he felt himself at liberty to attempt to realise some of those visions of retired and rural happiness, which had long played in his imagination, and which form, perhaps, one of the earliest reveries of every generous or cultivated mind. He began, therefore, immediately to embellish his grounds, to extend his plantations, and in the enlargement of his house, to render it more adequate to the purposes of hospitality; and in the course of a short period, he succeeded in creating a scene of rural and domestic happiness, which has seldom been equalled in this country, and which, to the warm-hearted simplicity of Scottish manners, added somewhat of the more refined air of classical elegance.

elegance. It was here, from this period, that all his hours of enjoyment were passed,—that all his works were composed,—and that, in the bosom of his family, and amid the scenery and amusements of the country, he found the happiness that was most congenial to his character and disposition.

His morning hours were uniformly given to study; but his studies were of a nature that tended rather to animate than to fatigue his mind. It was not in abstract or metaphysical speculations he was engaged, where the understanding only is exercised, and where the progress of discovery is so little proportioned to the time or labour that is employed; nor in works of imagination, where the mind is ever in pursuit of that ideal excellence which it is never destined to attain. The historical, the antiquarian, or the critical studies in which he was engaged, required no painful concentration of thought, and no laborious processes of reasoning. They related to the deeds and language of men, where it was not the understanding alone that was employed, but where the imagination and the heart were perpetually exercised; and he could rise from them to the common business or offices of life, with a mind undistracted by doubt, and unfatigued by abstraction. The employments to which he gave his hours of exercise, were of the same gentle and cheerful kind. He had little relish for the sports of the field, unless angling, in which, like the amiable and contemplative WALTON, he had from his early days delighted; but he took great delight in gardening, in the embellishment of his pleasure-grounds, and, more than all, in improving the dwellings, and extending the comforts of his cottagers,—an occupation, in which taste so fortunately combines with beneficence, and in which, for all the labour or expence he bestowed, Mr TYTLER found himself every day rewarded, by seeing the face of nature and of man brightening around him.

The

The society that assembled at his table, was the best that at that period this country afforded,—his own family-relations,—the families of the neighbouring proprietors in the populous county of Mid-Lothian,—most of the men eminent in science or in literature, of which our metropolis was then so profuse,—and occasionally those strangers of distinction, whom the love of science or of nature had induced to visit Scotland. His hospitality was cordial, but unobtrusive,—his attentions were so unostentatious, that his visitors found themselves at once at home,—and he himself appeared to them in no other light than as the most modest guest at his own table. The conversation which he loved, was of that easy and unpremeditated kind in which all could partake, and all enjoy. To metaphysical discussion, or political argument, he had an invincible dislike; but he gladly entered into all subjects of literature or criticism,—into discussions on the fine arts, or historical antiquities, or the literary intelligence of the day; and when subjects of wit or humour were introduced, the hearty sincerity of his laugh, the readiness of his anecdote, and the playfulness of his fancy, shewed to what a degree he possessed the talents of society. His sense of humour was keen, but at the same time characteristic: it was the *ludicrous*, rather than the *ridiculous*, in character or in manners, which amused him: those excesses rather of the amiable than of the selfish or sordid passions, which are observed with a sentiment of tenderness as well as of disapprobation, and which the poet has so happily expressed by the phrase, *circum præcordia ludit.* The humour of most men is unhappily mingled with qualities which add little to the amiableness, and still less to the respectability of character. From the gayest conversation of Mr Tytler, on the contrary, it was impossible to rise, without a higher sense of the purity of his taste, and the benevolence of his nature.

His evenings were always passed in the midst of his family, either in joining them in the little family concerts with which, like his father, he always wished to close the day, or in reading aloud to them some of those works by which he thought their tastes or their minds might be improved; or, not unfrequently, when none but his more intimate friends were present, in sharing with his younger children in those various youthful amusements which contribute so much to the gaiety of domestic life, and in which the affections of kindred, and the love of home, are so well, though so insensibly cultivated.

Of this scene of simple and virtuous happiness, there are some present who will not easily part with the remembrance, though accompanied with the melancholy reflection that they can meet it no more; and Mr Mackenzie will, I trust, forgive me for reminding him of an expression which he used to me many years ago, when I accidentally met him upon the road as he was returning from Woodhouselee, and which conveys so much better than any thing I can say, the character of the scene. "I hope," said he, "that you are going to Woodhouselee; for no man can go there without being happier, or return from it without being better."

To this picture, however, there is yet another feature to be added: it is in the sentiments with which Mr Tytler felt the prosperity he enjoyed. In the little MS. volume from which I have formerly quoted, (and from which I should more frequently quote, if I did not feel it a kind of profanation to expose to the eyes of the world that train of secret thought which was intended only for the eyes of his children), I find the following passage, for the introduction of which, I am sure I need no apology, and which expresses, in a manner which no biographer can do, the governing principles and persuasions of his mind. It was written on his birth-day, 15th October 1795.

"I

"I have this day (says he) completed my forty-eighth year, "and the best part of my life is gone. When I look back on "what is past, I am humbly grateful for the singular blessings "I have enjoyed. All indeed that can render life of value, "has been mine. Health, and peace of mind;—easy, and even "affluent circumstances;—domestic happiness;—kind and af-"fectionate relations;—sincere and cordial friends;—a good "name;—and, I trust in GOD, a good conscience. What "therefore on earth have I more to desire? Nothing; but if "He that gave, so please, and if it be not presumption in me "to pray,—a continuance of those blessings. Yet, if it should "be otherwise, let me not repine. I bow to His commands, "who alone knows what is best for his creatures; and I say "with the excellent GROTIUS,

> "Hactenus ista: latet sors indeprensa futuri:
> Scit, qui sollicitum me vetat esse, DEUS.
> Duc genitor me magne! Sequar, quocunque vocabor,
> Seu Tu læta mihi, seu mihi dura, paras.—
> Sistis in hac vita? Maneo, partesque tuebor
> Quas dederis. Revocas, Optime? Promptus eo."

The melancholy change for which Mr TYTLER seems thus to have prepared his mind, was soon to take place. In the autumn of the year 1795, he was seized with a long and dangerous fever, accompanied with delirium, and tending frequently to relapse. Under the anxious care of his friend and physician Dr GREGORY, he recovered from the fever; but in one of the paroxysms of the disease, he had the misfortune to rupture some of the blood-vessels of the bladder,—an accident which not only protracted his recovery at the time, but which threatened to degenerate into one of the most painful diseases to which the human frame is subject.

In the state of weakness and suffering which succeeded this severe illness, Mr Tytler was for a long time incapable of returning to his professional studies: but his mind was incapable of inactivity; and he turned willingly to those pursuits in natural history which had formed the amusement and the delight of his youth, and which are perhaps of all others the most suitable to the grateful feelings of convalescence.

Among the works with which he now amused himself, was the once celebrated treatise of Dr Derham, entitled *Physico-Theology.* In perusing it again, with all the affecting associations which the past and the present afforded him, he could not but lament, that it was in some degree rendered obsolete, by the innumerable discoveries with which science has been enriched since its publication, and that its popularity among those to whom it might be most serviceable, was restrained by the number of Latin quotations which remained without a translation. It occurred to him that his hours of convalescence could not be better employed than in remedying these defects, and in thus extending the usefulness of a work of which he had himself felt the value. This pleasing and unfatiguing task he executed with his usual ardour, and prefixing to it a short but valuable dissertation on Final Causes, published it in the year 1799.

Of this work, it is unnecessary for me to enter into any further detail; but I cannot omit a passage relating to it, which I find among Mr Tytler's papers, and which marks distinctly the great principle by which his studies as well as his conduct were governed.

" Of all my literary labours, (says he,) that which affords me
" the most pleasure on reflection, is the edition which I pu-
" blished of *Derham's Physico-Theology.* The account of the
" Life and Writings of Dr Derham, with the short disser-

" tation

" tation on Final Causes, the translation of the Notes of the " Author, and the additional notes, containing an account of " those more modern discoveries in the sciences and arts " which tend farther to the illustration of the subjects of the " work, are all the original matter of the edition to which I " have any claim; so that the vanity of authorship has a very " small share in the pleasure I enjoy from it. But when en- " gaged in that work, I had a constant sense that I was well " employed, in contributing, as far as lay in my power, to " those great and noble ends which this most worthy man " proposed in his labours, by enforcing on the minds of man- " kind the conviction of an all-wise and all-beneficent Author " of Nature. The demonstration, in short, of that great and " central truth, on which depends our present happiness and " our future hopes. Since the publication of this edition, " some other excellent works have appeared upon the same " subject, from which many valuable additions may be made " to the Notes on DERHAM, and I intend, accordingly, to make " those additions, if a new edition should be wanted in my " lifetime."

The year 1799 was distinguished by the agitation of the great question with regard to the Union with Ireland; and in attending to the debates it occasioned, Mr TYTLER thought that no view of the subject could be better fitted to conciliate the minds of the Irish people to this important measure, than a representation of the benefits which Scotland had derived from the Union with England. These observations he threw into the form of a letter; and they were published at Dublin, with the title of *Ireland profiting by Example; or the Question considered, Whether Scotland has gained or lost by the Union?* Of this little work it is enough to say, that such was its merit, or its popularity,

popularity, that three thousand copies were sold upon the day of its publication.

In the year 1801, a vacancy occurred in the Bench of the Court of Session, by the death of Lord Stonefield. The friendship of Lord Melville had a new opportunity for its display; and the friends of Mr Tytler had now the satisfaction of seeing him elevated to the highest honours of his profession. On the 2d of February 1802, he took his seat upon the Bench with the title of Lord Woodhouselee.

Of Lord Woodhouselee's qualifications for this important office, it would be presumptuous in me to offer any opinion; and I feel, with gratitude, that it is unnecessary, as, of all the honours which the Government of this country has to bestow, those which have been in the estimation of the public most purely won, and most honourably worn, are those which belong to the Administration of Justice. He brought not the Bench, indeed, either that profound acquaintance with the details of law, which nothing but continued and extensive practice can give; nor that metaphysical acuteness, which so often seeks to distinguish itself by subtlety of distinction, or novelty of interpretation; nor that impatient eloquence, which loves to find in the most trivial cases, an opportunity for its own display. But he brought to it qualities, in a country like this, of higher value, and of more genuine usefulness,—a just and enlightened admiration of the laws he was called to administer,—the most conscientious patience in the investigation of truth,—and a mind incapable either of being intimidated, in the discharge of duty, by the dread of censure, or of being misled by the love of praise. In his conduct on the Bench, the characteristic integrity and modesty of his nature were apparent. In this, as in all other situations, his highest ambition was to be *par negotiis, non supra*,—to be able to fulfil his duty without seeking for

for personal fame; and to accommodate his conduct, not so much to the opinion of men, as to that higher standard, which existed in his own breast. There were, however, occasions when his powers were more peculiarly called forth; and, upon some of these appearances from the Bench, there are many of us who can remember the high praise that was bestowed by the late Lord President BLAIR,—a man whose praise was fame, and who was of too proud an integrity to bestow it where he did not feel it was deserved.

From the period of his elevation to the Bench, Lord WOODHOUSELEE devoted his time exclusively, (while the Courts were sitting,) to the business that arose; but, during the vacations, he was always happy to return to his private studies. The solitude of the country, (to which he then always retired,) invited him to labour; and as he was now free from his academical engagements, and from that continued attention which the improvement of his lectures occasioned, he had time to return to the consideration of some of the literary projects which he had formed in his earlier days, and which he hoped he might now be able to resume. One of these, I find, was the literary and political Life of BUCHANAN; a subject which was interesting to him from many associations, and in which he proposed to do ample justice to his genius as a poet, and his merits as a historian, but to examine, with firmness and accuracy, his conduct as a man, and as a politician.

Another was to give a faithful translation of CAMDEN's *Annals of Elizabeth*, illustrated with notes, and comparing it with the best accounts of her time that have since been published. The subject had been suggested by Dr CAMPBELL in the *Biographia Britannica*, and in the view which Lord WOODHOUSELEE took of it, it promised him the opportunity of exhibiting a fuller and more faithful picture of that interesting period in English

English history, than had yet been accomplished in any one performance in our language. The most important, however, of these literary projects, was that of a continuation of Lord HAILES's *Annals of Scotland*, from the period when Lord HAILES's researches closed, to the accession of JAMES VI. to the Crown of England; a work to which no common talents were adequate, and of the difficulty of which no stronger evidence can be given, than that, however desired, it has yet remained unattempted.

All these projects, however, yielded to another, which was much more interesting to Lord WOODHOUSELEE himself, and to the accomplishment of which he was animated by something more than the hope of literary fame,—this was the Life of his earliest friend and patron Lord KAMES. "He had wait-" ed, (as he says,) with his usual modesty, for more than twen-" ty years, in the hope of its falling into abler hands." He was now raised to the same Bench which had been dignified by the presence of Lord KAMES; and the business in which he was engaged, served every day to bring him to his remembrance, and to afford him the new opportunities of appretiating his learning and his genius. From this fortunate concurrence of circumstances, Lord WOODHOUSELEE felt himself emboldened to undertake the task, and having determined upon his plan, he entered with eagerness upon the study of his works, and the collection of materials; and in the course of the vacations of only four years, he was able to accomplish his design. The work was finally published in two volumes, quarto, in the year 1807, with the title of *Memoirs of the Life and Writings of* HENRY HOME, *Lord* KAMES.

It is impossible not to admire the motives which led Lord WOODHOUSELEE to this undertaking, and it is impossible also not to respect the ability with which, amid the distractions of public

public business, and the sufferings of infirm health, he has been able to execute it; yet I know not if the friends of Lord WOODHOUSELEE's literary fame have not some reason to lament his choice of a subject; and there are circumstances in the extent and variety of Lord KAMES's powers, which seem to me to place him almost beyond the reach of the biographer.

The fortunate subjects of biography are those, where some powerful and uniform interest is maintained,—where great minds are seen advancing to some lofty and determinate object,—and where, amid the toils or the difficulties they have to encounter, the mind of the reader feels somewhat of the same anxious and unbroken interest, with which we follow the progress of the drama, or the narrative of the epic poet. The lives of conquerors, and of legislators,—of discoverers in science, or of inventors in the arts,—of the founders of schools in philosophy, or of sects in religion, it is impossible even for the rudest hand to trace, without awakening an interest which all men can understand, and in which all can participate; and even the history of inferior men can yet always be made interesting, when one object of ambition is seen to be steadily pursued, and one correspondent sympathy is awakened. Of this unity of pursuit, and of interest, the Life of Lord KAMES was singularly destitute. There was a vigour in his powers, and an elevation in his ambition, that were incapable of being restrained within the limits of any one pursuit; and he seems to have felt it to be his peculiar destiny, to take the lead in every science by which the reputation of his country could be exalted, and in every art by which its prosperity could be increased. To delineate the progress of such a mind; to follow his steps in all the various fields of inquiry through which he travelled,—to mark with precision the accessions that science derived from his labours, or the arts from his suggestions, was

a task to the execution of which, few men could bring adequate knowledge or capacity; and, even if it could have been executed, there were still fewer readers who could preserve any continuity of interest in a progress so eccentric, or be able to make perpetual transitions from the subtleties of metaphysics to the details of husbandry, or from the refinements of philosophical criticism, to the technical questions of Scotch law. The emblem of Lord KAMES's genius was not that of the Ganges or the Indus, which roll forward their condensed streams, and fill the eye of the spectator with their simple and increasing majesty; but that of the Rhine or the Nile, which divide the volume of their waters into innumerable branches, and, while they fertilize a wider surface, yet perplex the eye, that labours to number and pursue them. What fidelity and affection could do, upon a subject so difficult, Lord WOODHOUSELEE, I apprehend, has done. He has given the portrait of Lord KAMES, with all his various and characteristic features;—he has *surrounded him with his contemporaries*, and sketched out, in many pleasing and interesting details, the literary history of the age in which he lived;—and his work, like those of PLATO and of XENOPHON, will descend to posterity with an interest which no other can now possess, that of being executed from the living subject, and of blending the veneration of the disciple with the fidelity of the historian.

In the year 1811, Lord WOODHOUSELEE was appointed to the Justiciary Bench, on the elevation of the Lord Justice-Clerk HOPE to the President's chair.

Although Lord WOODHOUSELEE was now advancing in age, and his strength declining, yet the publication of the Memoirs of Lord KAMES did not put a period to his literary activity. It was now too late, indeed, for him to resume any of the literary projects which he had once hoped to accomplish: but he returned

returned willingly to another occupation, with which he had always intended to close his literary career. This was the revision of his lectures upon history. In the composition of these lectures, the best years of his life had been employed, and at the distance of time that had intervened, he was now able to review them with the eye of impartial criticism, and to make such additions or alterations as might better fit them for that general usefulness for which they were originally intended. To this pleasing occupation all his remaining seasons of leisure were devoted; and with the usual chearfulness of his temper, he flattered himself, that he might be able to accomplish a revision of the whole of the lectures that composed his Academical Course. As the first great subject of these lectures related to Grecian History, he now began anew the study of the Greek historians; and as his views included the history of science, of literature and of the fine arts, he was led insensibly to the study of the moralists, the orators and the poets, of that interesting period. So fascinating to his mind was the occupation, that, in the course of a few vacations, he was able to compose anew the whole of his lectures upon Grecian History, and to be rewarded by that peculiar delight, (which has been so often observed in the later years of literary men,) the delight of returning again to the studies of their youth, and of feeling, under the snows of age, the chearful memories of their spring.

In the year 1812, the death of his friend and relation General Sir JAMES CRAIG, (the late Governor of Canada,) and the property to which he succeeded by his will, rendered it necessary for Lord WOODHOUSELEE to undertake a journey to London. As Sir JAMES CRAIG had been distinguished by the Order of the Bath, it became the duty of Lord WOODHOUSELEE, as his nearest relation, to return to the Prince Regent the ensigns of

the Order; and for this purpose his Royal Highness was pleased to grant him an audience. Of this interview Lord Woodhouselee always spoke with gratitude, not only as it afforded him the opportunity of observing that dignified courtesy by which the manners of the Prince Regent are distinguished, but as it shewed him the intimate acquaintance which his Royal Highness possessed with regard to the affairs of Scotland, and the interest which he took in her progress in science and in literature. Some time after the interview with the Prince Regent, it was intimated to Lord Woodhouselee, that, if agreeable to him, the dignity of Baronet would be conferred on him, which he requested permission to decline,—an instance of modesty, which surprised no one to whom Lord Woodhouselee was known; and which (I am proud to say) was to none so acceptable as to his own family, to whom no illustration could be so dear as that of their father's name.

I am led, besides, to mention this journey of Lord Woodhouselee to London, as it gives me the opportunity of introducing a little composition to which it gave occasion, and which ought not to be omitted in any account of his life. He had for some time believed, that the disease under which he laboured was soon to be fatal; and a little before this, he had given orders that his family burial place should be repaired, and had inscribed upon it an epitaph, full of tenderness and of affection, to the memory of his father and his mother. In leaving London for the last time, and returning to his own country, it was natural for him to look forward to the event which he had long thought approaching, and to that final home where he was to rest with his fathers. Under these impressions the following lines were composed, as he was returning homewards; and as they afford a picture of his mind which no Biographer could reach,

reach, I trust I need no apology for introducing them in this place.

The Verses are entitled:

In Sepulchrum meum avitum, in Cemeterio Franciscонorum, Edinburgi, nuper re-ædificatum.

Jam duodecimum condere lustrum
Contigit,—et jam cernere canos
Vertice summo, dum fatiget
Impigra quondam membra gressus,
Nec oculi vigeant nec aures,
Hebeat et prorsus sensuum acumen.—
—Hæc sunt nec tardæ indicia mortis.—
Hisce admonitus Fati nunciis,
Refici avitum denuo Sepulchrum
Curo, et cineris protegi injuria
Mistæ ut amicis reliquiis cubent,
—Hic enim juncti quondam vita
(Nec sivit divelli fatum),
Dormiunt una Pater et Mater,
Purusque et pius ordo Parentum.——
——Salve! O vitaï Anchora et Portus!
Salve! laborum terminus et quies!
Salve! brevi subeundaque tecta
Hospitium viatori fesso!
Te specto impavidus; te longam
Per noctem fidus sis custos,
Et reddas (precor) incolumem DEO.

The event to which Lord Woodhouselee thus steadily looked forward was now approaching. In June 1812, after superintending

intending his workmen in some improvements he was making at Woodhouselee, he felt that he had fatigued himself, and he was soon sensible of the recurrence of the same unfortunate accident which had laid the foundation of so many years of suffering. From this period, the remainder of his life was a scene of continued pain and increasing debility,—borne, indeed, with the most calm and even chearful resignation, and relieved by every thing that filial and conjugal tenderness could supply, yet too visibly approaching to a period which neither tenderness nor magnanimity could avert.

In the beginning of winter, he was prevailed upon to leave his favourite Woodhouselee, and to remove into town; and from this time his disease appeared to make a more rapid progress. On the 4th of January 1813, he felt himself more than usually unwell; and in the evening, when his family, with their usual attentions, were preparing to read to him some work of amusement, he requested that they would rather read to him the evening service of the Church, and that they might once more have the happiness of being united in domestic devotion. When this was finished, he spoke to them with firmness, of the events for which they must now prepare themselves: He assured them that to him death had no sorrow but that of leaving them: He prayed that Heaven might reward them for the uninterrupted happiness which their conduct and their love had given to him; and he concluded, by giving to each of them his last and solemn blessing.

After the discharge of this last paternal duty, he retired to rest, and slept with more than his usual tranquillity, and in the morning, (as the weather was fine,) he ordered his carriage, and desired that it might go out on the road towards Woodhouselee. He was able to go so far as to come within sight of

his

his own grounds; and then raising himself in the carriage, his eye was observed to kindle as he looked once more upon the hills, which he felt he was so soon to leave, "*and which he* "*had loved so well.*" There was an influence in the scene which seemed to renew his strength, and he returned to town, and walked up the stair of his house with more vigour than he had shewn for some time; but the excitement was momentary, and he had scarcely entered his study, before he sunk down upon the floor, without a sigh or a groan. Medical assistance was immediately procured, but it was soon found that all assistance was vain; and Dr Gregory arrived in time only to close his eyes, and thus to give the final testimony of a friendship which, in the last words that he wrote for the press, Lord Woodhouselee had gratefully commemorated as having *borne the test of nearly half a century.*

His remains were interred in the family burial-place in the Grayfriars Church-yard, beside those of his father and mother, to whose memory it was then found, that his filial piety had so exclusively dedicated it, that their epitaph occupied the whole of the tablet, and no room was left for any inscription to himself.

I have very ill executed the melancholy task I have undertaken, if it is now necessary for me to conclude this account with any laboured delineation of the character of Lord Woodhouselee. I am speaking to some, in whose memories his virtues are written in better characters than those of words; and I am too conscious of the partiality of friendship, to trust myself to any other representation than that which his own life

and

and conduct can supply. Upon his literary character, it will be the province of posterity to pronounce; and to it I willingly leave to determine the rank he is to hold among the writers of his country. To us in these moments, when we are again, as it were, leaving his grave, there are other reflections that belong; and there are recollections of no vulgar kind that arise, when we review the life of which we have seen the close.

It was a life, in its first view, of usefulness and of honour. He was called to fill some of the most important offices which the constitution of human society affords,—as a father of a family,—a possessor of property,—a man of letters,—and a Judge in the Supreme Courts of his country; and he filled them all, not only with the dignity of a man of virtue, but with the grace of a man whose taste was founded upon high principles, and fashioned upon exalted models. It was a life, in its second view, of happiness as well as of honour: happy in all the social relations which time afforded him,—in the esteem of his country,—the affection of his friends,—the love and the promises of his children: happy in a temper of mind which knew no ambition but that of duty, and aspired to no distinction but that of doing good: happier than all in those early and elevated views of Religion, which threw their own radiance over all the scenes of man or of Nature through which he passed, and which enabled him to enjoy every present hour with thankfulness, and to look forward to every future one with hope.

The records of this Society contain the histories of greater men,—of none, I believe, more virtuous, more amiable, or more happy: And while the lives of these illustrious men, (written

(written by men of kindred genius,) will, I trust, long continue to inspire in this place the spirit of philosophical ambition, I dare to hope, that even the faint outline which I have now given of the character of Lord Woodhouselee, may tend to cherish that *moral* ambition which all men are called to indulge; without which learning is vain, and talents are dangerous, and to which rewards of a nobler kind are assigned, than either the praise of men or the splendors of literary fame.

APPENDIX.

Containing Lists of the Office-Bearers and Members elected since November 1815.

November 1815.

OFFICE-BEARERS.

Sir James Hall, Baronet, President.

Lord Meadowbank, } Vice-Presidents.
Right Honourable Lord Gray, }

Professor Playfair, Secretary.

James Bonar, Esq. Treasurer.

Thomas Allan, Esq. Keeper of the Museum and Library.

PHYSICAL CLASS.

Sir George Mackenzie, Baronet, President.

Thomas Charles Hope, M. D. Secretary.

Counsellors.

James Bryce, Esq.

David Brewster, LL. D.

Sir William Forbes, Baronet.
Andrew Coventry, M. D.
Professor Leslie.
Lord Webb Seymour.

LITERARY CLASS.

Henry Mackenzie, Esq. President.
Thomas Thomson, Esq. Secretary.

Counsellors.

Reverend Dr Jamieson.
Walter Scott, Esq.
Lord Glenlee.
Dr Thomas Brown.
William Arbuthnot, Esq. Lord Provost.
James Pillans, Esq.

22d January 1816.

MEMBERS ELECTED.

Captain Thomas Colby, Royal Engineers.
Leonard Horner, Esq. F. R. S. Lond.
Henry Colebrooke, Esq.
Rev. George Cook, D. D. Laurencekirk.
Right Honourable William Adam, Lord Chief Commissioner.
John Fullerton, Esq. Advocate.

Thomas

THOMAS JACKSON, LL.D. Professor of Natural Philosophy, St Andrew's.
JOHN ROBISON, Esq.
HENRY DEWAR, M. D.
Mr HUGH MURRAY.
Mr ROBERT WILSON, Accountant.

November 1816.

OFFICE-BEARERS.

Sir JAMES HALL, Baronet, President.
Lord GLENLEE, } Vice-Presidents.
Right Honourable Lord GRAY, } Vice-Presidents.
Professor PLAYFAIR, Secretary.
JAMES BONAR, Esq. Treasurer.
THOMAS ALLAN, Esq. Keeper of the Museum and Library.

PHYSICAL CLASS.

Sir GEORGE MACKENZIE, Baronet, President.
THOMAS CHARLES HOPE, M. D. Secretary.

Counsellors.

Sir WILLIAM FORBES, Baronet.
ANDREW COVENTRY, M. D.
Professor LESLIE.
Lord WEBB SEYMOUR.
Colonel IMRIE.
Professor JAMESON.

LITE-

LITERARY CLASS.

HENRY MACKENZIE, Esq. President.
THOMAS THOMSON, Esq. Secretary.

Counsellors.

Dr THOMAS BROWN.
WILLIAM ARBUTHNOT, Esq. Lord Provost.
JAMES PILLANS, Esq.
Professor DUNBAR.
Reverend Dr MACKNIGHT.
Reverend ARCHIBALD ALISON.

27th January 1817.

MEMBERS ELECTED.

The Honourable Baron CLERK RATTRAY.
Right Honourable the Earl of WEMYSS and MARCH.
The Honourable DAVID DOUGLAS, Lord Reston.
FRANCIS BUCHANAN, M. D. F. R. S. Lond.
JOHN WILSON, Esq. Advocate.
DAVID HOSACK, M. D. F. R. S. New York.
Right Honourable ALEXANDER MACONOCHIE, Lord Advocate.
JOHN FLEMING, M. D. late of Calcutta.
DAVID JOHN HAMILTON DICKSON, M. D. Clifton.
WILLIAM P. ALISON, M. D.
JAMES SKENE, Esq. of Rubislaw.
Dr HOWEL.

Rev.

Rev. Robert Morehead.
Robert Bald, Esq. Civil Engineer.
Thomas Sivright, Esq. of Meggetland.

November 1817.

OFFICE-BEARERS.

Sir James Hall, Baronet, President.

Lord Glenlee, }
Right Honourable Lord Gray, } Vice-Presidents.

Professor Playfair, Secretary.

James Bonar, Esq. Treasurer.

Thomas Allan, Esq. Keeper of the Museum and Library.

PHYSICAL CLASS.

Sir George Mackenzie, Baronet, President.

Thomas Charles Hope, M. D. Secretary.

Counsellors.

Lord Webb Seymour.
Professor Leslie.
Colonel Imrie.
Professor Jameson.
David Brewster, LL. D.
Mr James Jardine.

LITE-

LITERARY CLASS.

HENRY MACKENZIE, Esq. President.

THOMAS THOMSON, Esq. Secretary.

Counsellors.

JAMES PILLANS, Esq.
Professor DUNBAR.
Reverend Dr MACKNIGHT.
Reverend ARCHIBALD ALISON.
Lord RESTON.
Rev. Dr JAMIESON.

26th January 1818.

MEMBERS ELECTED.

THOMAS MACKENZIE, Esq. younger of Applecross.
WILLIAM RICHARDSON, M. D. Harrowgate.
Honourable Captain WILLIAM NAPIER, R. N. of Merchistoune.
HARRY WILLIAM CARTER, M. B. Oxford.
PATRICK MILLER, M. D. Exeter.
NATHANIEL BOWDITCH, Esq. Salem, Massachusetts.
JOHN CRAIG, Esq.
JOHN WATSON, M. D.
Captain THOMAS BROWN.
JOHN HOPE, Esq. Advocate.
Major JAMES ALSTON.
WILLIAM FERGUSON, M. D. Inspector of Hospitals.
Sir WILLIAM HAMILTON, Baronet, Advocate.

END OF VOL. VIII.